Journal of
Experimental Psychology

VOL. 20, NO. 5 MAY, 1937

CHARACTERISTICS OF DELAYED AND TRACE CONDITIONED RESPONSES [1]

BY ELIOT H. RODNICK

Institute of Human Relations,
Yale University

INTRODUCTION

Although the development of delayed conditioned responses has been described by Pavlov (**18**) with dogs and more elaborately by Switzer (**21**, **22**) with humans, there is no comparable description of trace conditioned responses.[2] Steckle (**20**) reported some indication of the development of very short trace conditioned reactions with the galvanic skin response in adult humans. However, neither this nor Leonow's (**15**) study on human infants presents any data concerning trace conditioned response phenomena that would permit a comparison with existent data on delayed conditioned responses.

[1] The writer wishes to express his deep appreciation to Professor Clark L. Hull, in whose laboratory this study was performed, for his many helpful suggestions. This is part of an investigation presented to the Faculty of the Graduate School of Yale University in partial fulfillment of the requirements for the degree of Doctor of Philosophy in psychology.

[2] In the delayed conditioned response situation, the conditioned stimulus persists for an appreciable interval of time before the reinforcing or unconditioned stimulus is presented (see Fig. 1). On the other hand, in the trace situation the conditioned stimulus is separated from the unconditioned stimulus by a 'blank' period of no stimulation (Fig. 3). In this case the conditioned stimulus has been considered to be a 'trace' of the actual stimulus (**18**, 40). The more customary conditioned response is labeled simultaneous, since the onset of the conditioned stimulus does not precede the unconditioned stimulus by more than a very brief interval of time.

In view of the widely assumed identification of the properties of delayed and trace conditioned responses (3, 12, 13, 18), a comparison of these two types of conditioned reactions is essential. This is especially urgent in view of the publication of an hypothesis (16, 17) identifying the phenomena of trace conditioned reactions with the remote excitatory tendencies of Ebbinghaus (2, 4, 25) and the elaboration of numerous deductions based on this hypothesis concerning rote learning of nonsense syllables (11, 13). Even though the remote excitatory tendencies are analogous to trace conditioned responses, the bases for the deductions resulting from this hypothesis have of necessity been drawn almost entirely from the work on delayed conditioned responses, under the working hypothesis that the two exhibit the same properties. The present investigation provides a partial test of this particular assumption, through a comparison of the phenomena of these two types of conditioned responses.

I. Delayed Conditioned Response Experiment

Apparatus and Procedure

In this experiment a delayed conditioned galvanic skin response was set up to a visual stimulus in twenty male university students. They were paid by the hour. The subjects were seated in a semi-reclining position in a Morris chair, completely enclosed in a darkened, black cardboard cabinet. The continuous hum in the subjects' ears from a pair of headphones supplied with a 6-volt, 60-cycle A.C. current effectively eliminated the perception of slight noises made by either the experimenter or the apparatus. The use of absorbent cotton in the subjects' ears was an added safeguard which reduced the intensity of both the headphone hum and possible auditory cues.

The conditioned stimulus for the delayed galvanic skin response was a patch of faint light from a 6-volt flashlight bulb mounted in the front panel of the cabinet directly on a level with the subject's eyes. This light was dimmed by a layer of black sateen and had a duration of 21.3 seconds.

A moderate tetanizing shock delivered through two polished silver 25-cent pieces strapped to the right wrist served as the unconditioned stimulus. The current was derived from a 6-volt lead storage battery and passed through a Cambridge inductorium. The intensity of the shock was adjusted by varying the amount of overlap between the primary and secondary coils of the inductorium and was sufficient to cause a visible contraction of the muscles of the forearm in most of the subjects. The shock had a duration of .625 second and was presented 20.1 seconds after the onset of the light so that the latter continued for about .6 second beyond the shock. The transcript of a typical record presented in Fig. 1 shows the temporal relationship between the shock and light stimuli. The delivery of these stimuli was controlled by a noiseless synchronous motor timer of special construction.

The device for recording the Tarchanoff phenomenon of the galvanic skin response has been described elsewhere (1). Two dry silver electrodes the size of quarter-dollars

were strapped about one-fourth inch apart against the palm of the left hand and were connected to a D'Arsonval galvanometer having a period of 6 seconds and a sensitivity of 0.0005 microampere. The galvanometer movements were traced with a 4 to 1 reduction on moving stylographic paper.

Following the experience of other investigators (18, 21, 22) in facilitating the development of the delayed conditioned response, three presentations of a 'simultaneous' light-shock combination preceded the 21-second light-shock stimulations. On the 'simultaneous' stimulations, the duration of the light was 2.3 seconds, the onset preceding the shock by approximately 1.1 seconds.

In order to prevent temporal conditioning, the paired stimuli were presented at intervals varying from 1 to 3 minutes and averaging 2 minutes. The following is an outline of the procedure employed for a single session of two and one-half hours' duration:

10 presentations of light alone prolonged for 21.3 seconds.
3 presentations of 'simultaneous' light-shock combination.
20 presentations of persisting light-shock combination.
15-minute rest interval
(subject allowed to exercise)
28 presentations of persisting light-shock combination.

Results and Discussion

The galvanic skin response to the 21-second light-shock combination exhibits the frequently observed characteristic of conditioned responses to antedate or anticipate the point of reinforcement (5, 7, 18, 21, 22). The transcript of a typical record showing a delayed conditioned response is presented in Fig. 1.

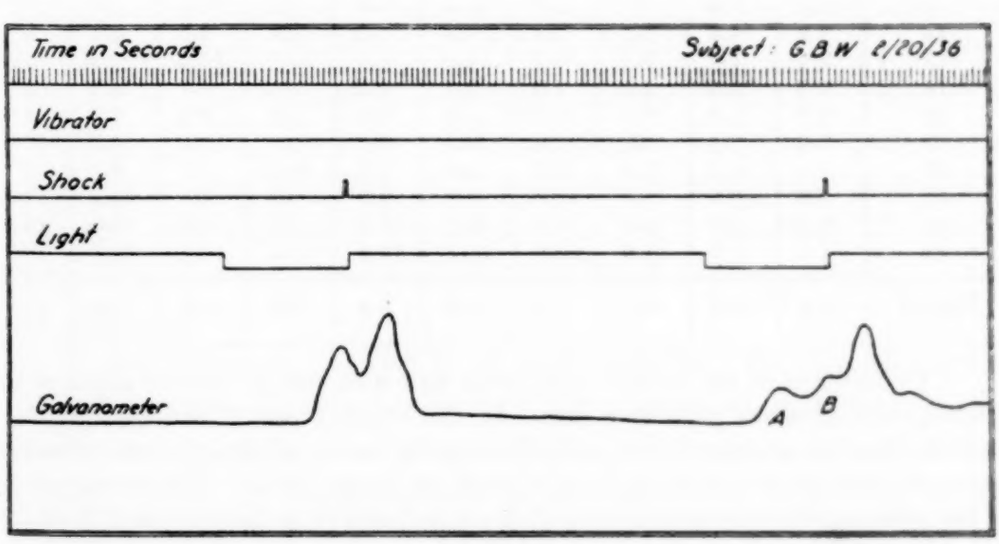


FIG. 1. Transcript of a section of a typical record (taken from Subject 5) showing a delay in the conditioned galvanic skin response to a persisting visual stimulus. At the left is shown a delayed conditioned response which consists of a single reaction; the anticipatory reaction at the right consists of a dual wave (*A* and *B*).

The progressive modification of the latency of the galvanic skin response to the reinforced persisting stimulus is shown in Table 1. It may be seen that the mean latency of the anticipatory response for 19 of the 20 subjects [3] increases from 4.4 seconds for the first five stimuli presentations to a mean of

TABLE 1

SUMMARY OF AVERAGE LATENCIES IN SECONDS OF THE CONDITIONED GALVANIC SKIN RESPONSE TO THE 21-SECOND VISUAL STIMULUS AT VARIOUS STAGES OF TRAINING

Reinforcement by shock occurred twenty seconds after the onset of the light. All twenty subjects received ten 21-second light stimuli alone and three simultaneous light-shock stimulations prior to the 21-second light-shock combinations. A 15-minute rest period during which the subject was allowed to exercise was interposed between trials 20 and 21.*

Subjects	Trials									
	1–5	6–10	11–15	17–20	21–25	26–30	31–35	37–39	41–44	46–47
1	5.0	6.2	10.6	7.2	6.4	16.1	12.0	11.4	14.3	8.8
2	6.8	6.7	4.1	10.4	8.1	12.6	8.2	8.6	11.0	11.8
3	4.6	4.1	6.2	7.1	4.7	8.7	7.1	9.2	9.4	8.6
4	4.6	12.7	8.6	16.4	12.6	11.0	11.7	12.4	10.3	17.5
5	3.6	8.4	5.6	4.1	9.1	11.9	10.1	12.1	12.9	8.7
6	3.5	9.1	5.5	13.2	13.6	8.0	12.2	10.2	8.9	13.0
7	4.9	12.5	6.0	5.2	10.8	6.9	7.7	14.4	9.2	6.3
8	4.1	8.5	4.3	6.9	7.0	4.4	4.4	7.8	4.9	14.0
9	4.1	5.5	3.8	3.8	5.6	6.9	9.5	6.6	5.9	9.3
10	3.8	11.1	8.7	6.4	6.5	8.6	7.0	9.5	10.1	6.5
11	13.5	6.1	†	5.5	11.1	6.8	3.2	10.3	5.2	†
12	3.4	3.4	3.5	3.1	5.6	3.0	4.1	3.8	4.1	5.3
13	4.0	4.3	8.5	6.7	5.0	5.1	10.5	10.9	7.2	4.9
14	2.7	5.9	4.2	3.9	7.3	4.0	4.6	7.1	4.3	4.7
15	3.7	5.7	7.1	7.0	5.4	9.6	7.7	7.5	7.8	8.9
16	6.9	5.3	10.3	5.4	8.8	8.7	9.0	12.6	5.8	10.9
17	4.4	4.5	3.4	5.5	4.0	9.5	10.2	5.3	8.5	9.1
18	2.6	7.6	3.6	10.0	6.7	3.4	9.7	5.1	14.4	11.0
19	5.3	3.6	4.1	9.1	6.3	8.2	7.2	7.8	9.6	16.5
20	5.5	4.3	13.0	10.6	13.4	10.2	11.0	10.4	11.1	15.7
Mean‡	4.4	6.8	6.4	7.5	7.7	8.3	8.6	9.1	8.9	10.1

* The subjects in the present experiment were also used in another phase of the investigation discussed elsewhere (19). In this other phase of the experiment, a tactual-vibratory stimulus and a puff of air to the cornea of the eye were presented 5 seconds after the onset of the light on trials 16, 36, 40, 45, and 48. The measurements of the galvanic skin response on these trials are excluded from the treatment of results.

† No conditioned reactions were obtained during these stimulations.

‡ 19 subjects. Subject 11 excluded.

[3] The data of Subject 11 were excluded from the computation of the mean value, since no measures were obtained for two points of the gradient.

10.1 seconds for trials 46 and 47, an increment of 5.7 seconds. This increase in latency of the delayed conditioned response with continued reinforcement confirms Switzer's (**21**) results. The gradients for the individual subjects are very variable, although most of the cases manifest the general trend exhibited by the mean value.

In order to determine whether the 15-minute 'rest' period interposed between trials 20 and 21 of the light-shock combination caused a shortening of the latency of the anticipatory response, the difference in the mean latency of the galvanic skin response for the two trials before and after the interval was obtained. The mean latency for 18 subjects [4] shows a drop from 8.11 seconds for trials 19 and 20 to 6.57 seconds for trials 21 and 22, a decrease of 1.54 seconds. This is a reduction of 42 percent in the increase of the latency of the delayed galvanic skin response due to the previous training. The not too satisfactory critical ratio of 1.58 based on the standard error of the difference represents about 94 chances in 100 that the difference is a true one. Switzer observed that a 24-hour interval caused the 'anticipatory reaction to move forward to, or very close to, its original position' (**21**). In view of the fact that in the present experiment the anticipatory response returned over 40 percent of the distance to its original position after only a 15-minute period, it appears quite plausible that the curve of the shortening of delay with the passage of time may show the maximum change during intervals very close to the point of the last reinforcement. On the other hand, with these limited data it is difficult to deny the possibility that this shortening of the latency is not a result of 'disinhibition' due to the subject's removal from the apparatus during the interval.

It was observed that a second reaction during the delay period occurred in 32 percent of the cases, so that the tracing of the anticipatory response appeared frequently as a dual curve. Such a double conditioned reaction is shown at the extreme right in Fig. 1. This phenomenon was reported also

[4] Of the two remaining subjects, one showed no conditioned reactions to these stimulations. Failure to restart the polygraph in time prevented the recording of the response on these trials with the other subject.

by Switzer (22). The increment in the mean latency of these secondary reactions for successive fourths of the training trials is shown in the following table:

	Stimulations			
	1–11	12–23	24–34	35–47
Mean latency.....	14.88 sec.	16.08 sec.	15.54 sec.	17.73 sec.

This modification of the latency of the secondary responses may be explained by the following hypothesis. The conditioned response may tend to recur as long as the conditioned stimulus pattern is present. In such a case, the second response would be prevented from occurring immediately after the first response by the refractory phase of the latter. If the length of interval between the two responses is constant, any increment in the latency of the first response would be reflected in an increase in the latency of the second response.

The question arises as to whether the developmental process of the delay of the anticipatory response involves one of two possible mechanisms, either one of which might account for a gradual increase in latency with training. First, there may actually be a gradual moving back of the conditioned response toward the point of reinforcement. Second, since the anticipatory reaction frequently consisted of two responses (a short-latency and a long-latency response) the initial short-latency response may gradually become extinguished, leaving the longer latency response near the point of reinforcement. The increase in the mean latency of the anticipatory response in the latter case may then be due to the fact that responses of a much longer latency have been averaged together with the short-latency responses. If the frequency of the long-latency responses progressively increased at the expense of the short-latency responses, there would appear to be a gradual lengthening of the mean latency. In investigating this possibility, the latencies of all the anticipatory responses were plotted as shown in Fig. 2 in order to determine whether they tended to fall into two groups. It may be seen that the curve shows a positively skewed distribution, instead of the bimodal

or possibly trimodal curve which would be expected were the above hypothesis correct in an extreme degree.[5] Individual results for each subject indicate that only two subjects showed fairly definite bimodal distributions. Two other distributions were normal; all the rest were skewed. Thus in most cases the latency of the delayed conditioned reaction apparently increases progressively with training. This was also supported by data obtained in the following experiment on trace conditioned responses.

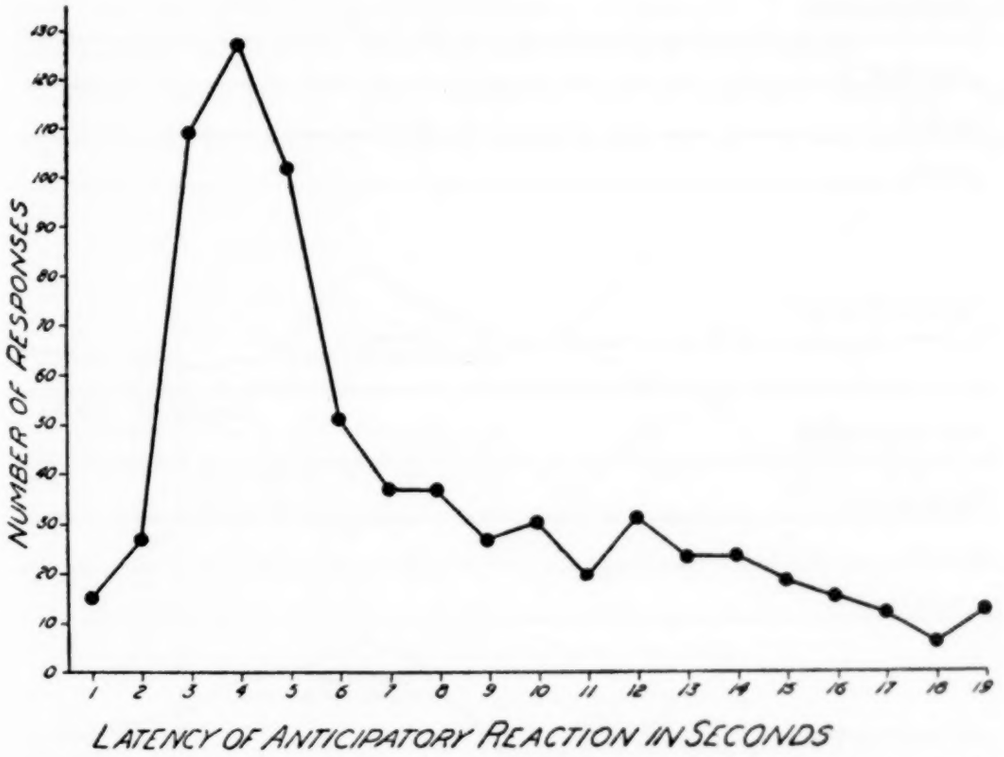


FIG. 2. The distribution of the latencies of the delayed conditioned galvanic skin response. This curve is derived from a total of 721 reactions obtained from 20 subjects.

II. TRACE CONDITIONED RESPONSE EXPERIMENT

Subjects and Procedure

Five male university students were employed as subjects for a period varying from 20 to 33 experimental sessions of one

[5] It is quite possible, of course, that the skewed curve is a resultant of two distributions which have a considerable amount of overlap. If that were actually the case, the mean of the second distribution would have to fall somewhere between 8 and 12 seconds (see Fig. 2). But since the mean of the secondary responses falls between 15 and 18 seconds, it does not appear likely that the skewed distribution is caused by these secondary conditioned responses.

hour each. They were paid for their services. A tactual-vibratory stimulus (1) having a duration of .180 second was followed after an interval of 17.4 seconds by faradic stimulation lasting .180 second.[6] All of the subjects received 20 reinforcements of the vibrator each day for the first 11 days, at intervals varying from 60 to 140 seconds and averaging 100 seconds. At the end of this 11-day period the number of reinforcements was reduced to 12 on certain days (according

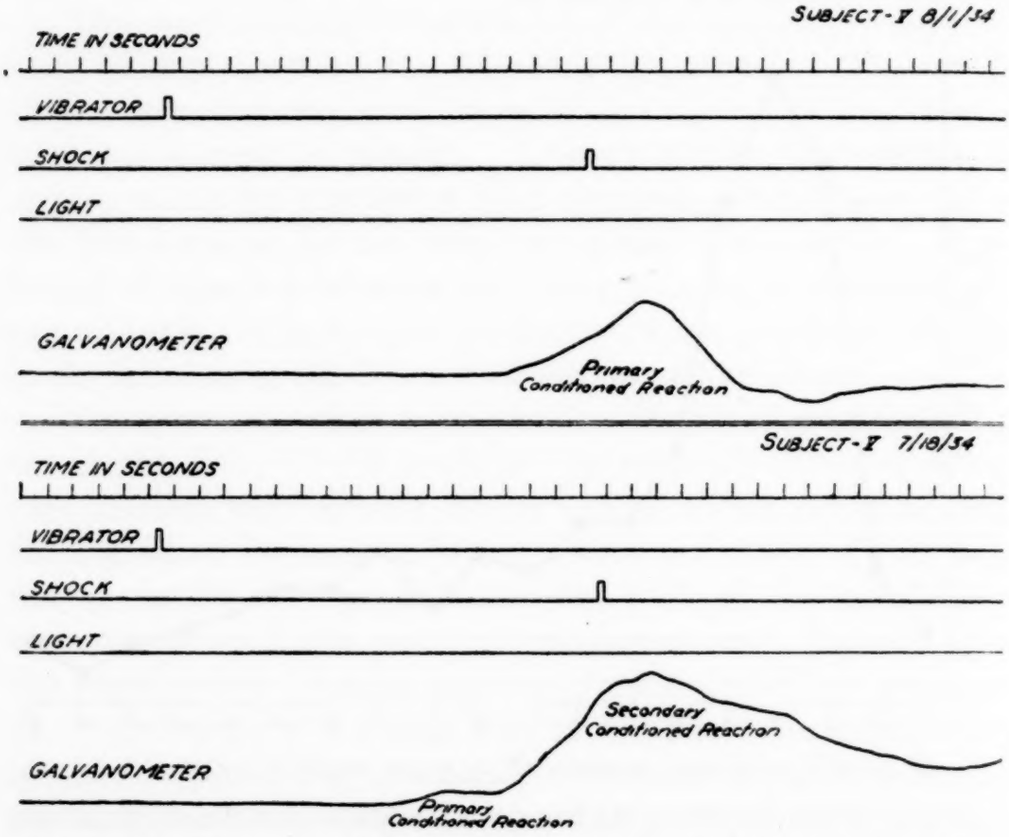


FIG. 3. Transcripts of typical records showing a developed trace conditioned response. The upper frame shows a trace conditioned response late in the training trials. This anticipates the shock by only four seconds. Note that the trace reaction and the response to the shock merge to form a single reaction. The lower frame shows a trace conditioned response which consists of a dual reaction.

to the individual subject) when, as a test of the inhibition of delay, a visual stimulus for a conditioned eyelid reaction followed the vibratory stimulus on three unreinforced stimulations. However, the results of this phase of the experiment will not be reported here.

[6] The shock was somewhat stronger than that employed in the preceding experiment.

Results and Discussion

It was observed that the course of the modification of the latency of the conditioned galvanic skin response was similar to that of the delayed conditioned response, although the rate was considerably slower.[7] Figure 3 shows transcripts of typical records of a single subject, taken in the later stages of training. As shown in this figure, the anticipatory response frequently merged with the unconditioned response to the shock, forming a single response. This tendency was much rarer in the preceding experiment, although it was reported by Switzer (**22**) with delayed conditioned responses.

Since not all of the subjects were carried beyond 20 experimental sessions, Fig. 4 shows the progressive modifica-

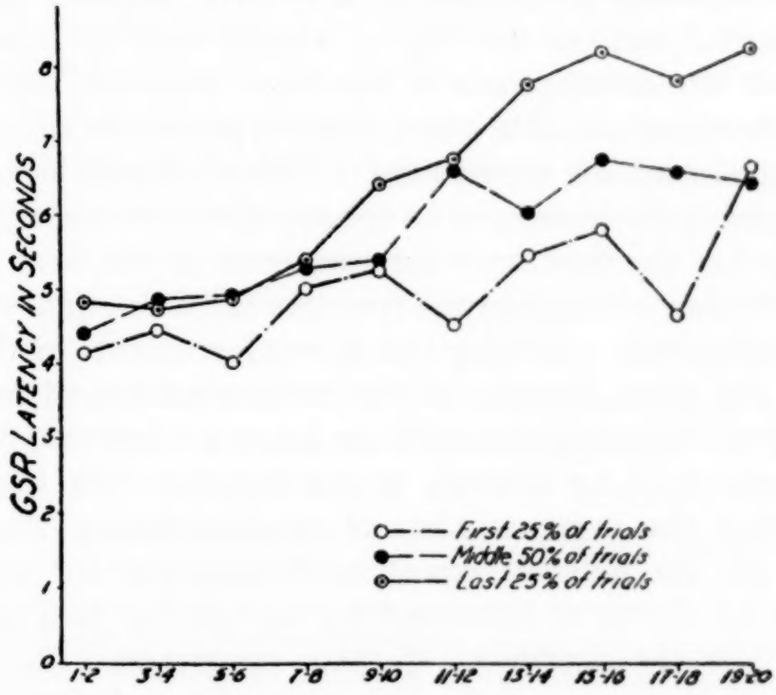


FIG. 4. Composite graph showing the modification of the trace conditioned response for successive amounts of each day's trials. Each point on the middle curve represents a mean of about 100 measures; each point on the other two curves represents a mean of about 50 measures.

tion in the latency of the primary anticipatory response for only the first 20 days of the experiment. This latency rises

[7] In both experiments the galvanic skin response to the to-be-conditioned stimulus was extinguished prior to the conditioning trials.

gradually from 4.2 seconds for the first 25 percent of the stimulations of days 1 and 2 to a maximum of 8.3 seconds for the last 25 percent of the stimulations of days 19 and 20, an increase of 4.1 seconds after about 374 [8] reinforcements.

Reference to Table 1 shows that this increase of 4.1 seconds in the latency of the delayed conditioned response was reached at approximately the 33rd reinforcement. Thus the trace conditioned response required about 11 times as many reinforcements to reach the same criterion of delay as did the delayed conditioned response.

However, this value of the relative difficulty of setting up the delay in these two types of conditioned responses should be larger than the true difference. Presumably the spontaneous recovery in latency of the trace response between experimental sessions (*cf.* Fig. 4) should tend to slow down somewhat the development of the trace response, thus introducing an added variable which was not present in the delayed conditioned response experiment. This error may be avoided by comparing the increment in latency of the trace conditioned response for the first 20 reinforcements of the first session only with that of the delayed response after an equal number of reinforcements. During the 20 reinforcements of the first session, the mean latency of the trade conditioned response for the five subjects increased from 4.0 to 4.3 seconds, which is an increment of 0.3 second, or 7.5 percent. On theo ther hand, after the same number of reinforcements, the mean latency of the delayed conditioned response for nineteen subjects (*cf.* Table 1) increased 3.1 seconds, or 70.4 percent. *Thus, under the conditions of these experiments, the rate of increase of the latency of the delayed conditioned response was about 9 to 10 times more rapid than that of the trace conditioned response.*

Some indication of this difference between the two types of conditioned responses has been suggested by Pavlov (18, 92) who states that the delay for the same stimulus develops more rapidly when it is continuous than when it is inter-

[8] This value represents the mean number of reinforcements for all 5 subjects. On some days only 12 reinforcements were given to certain subjects, instead of the customary 20.

mittent. The present results indicate that the situation involved in the establishment of a delay in conditioned responses is not merely a function of the temporal interval alone between the conditioned and unconditioned stimuli. This would follow from the fact that the length of the time interval was approximately the same in both experiments. Since the only important difference [9] between the two cases was the prolongation of the conditioned stimulus in one of them, the development of the delay is functionally related to the stimulation during the interval between the onset of the conditioned stimulus and the point of reinforcement.[10]

Further reference to Fig. 4 reveals that throughout the experiment there is a definite shortening of the delay period after a 24-hour interval. The mean latency of the trace conditioned response for the first 25 percent of each day's reactions is shorter at almost every point in the curve than the mean latency of the last 25 percent of the preceding session. Thus, while the curve for the last 25 percent of each session's trials shows a marked rise during the course of the experiment, the curve for the first 25 percent of the reactions rises comparatively slowly. It may be seen that the means for the middle 50 percent of each day's reactions occupy a position roughly midway between the other two curves. The latency increases during the training trials of a single session, but

[9] The present experiment actually preceded the delayed conditioned response experiment. In the latter experiment the vibratory stimulus was tried at first, but since the solenoid of the stimulator overheated when its activity was prolonged for as much as 20 seconds, the sense modality had to be changed to the visual field. Although this difference in modality may account for some of the differential in the rate of development of delay in the two experiments (18), it does not appear likely that the greater part of this striking difference could be due to this. The other differences in conditions in the two experiments had opposing effects so that they should have tended to counterbalance one another. The somewhat shorter interval between the conditioned stimulus and the shock in the present experiment was offset by the three simultaneous light-shock combinations preceding the persisting light reinforcements. The difference in duration of the shock was opposed by a difference in intensity, so that the net strength of the shock may have tended toward equality.

[10] Steckle (20) reports a more rapid development of a very short trace conditioned response with the use of a 5-second interval between conditioned and unconditioned stimuli. But in view of his method of recording the galvanic skin response only during extinction trials, a comparison with the present experiment is precarious.

returns close to its original position at the start of the next session.

The divergence of the curves for the first and last 25 percent of each day's trials indicates that the increase in latency during the course of a single session is much greater during the latter part of the experiment. This trend is shown in Fig. 5. Here the difference between the mean latencies of the trace conditioned response of the first and last 25 percent of each day's trials was plotted for successive groups of experimental sessions. It may be seen that for the first eight sessions this daily increase is very slight. There is then a rapid rise, followed by a leveling out after sixteen sessions; at this point the mean daily increase is 2.38 seconds.

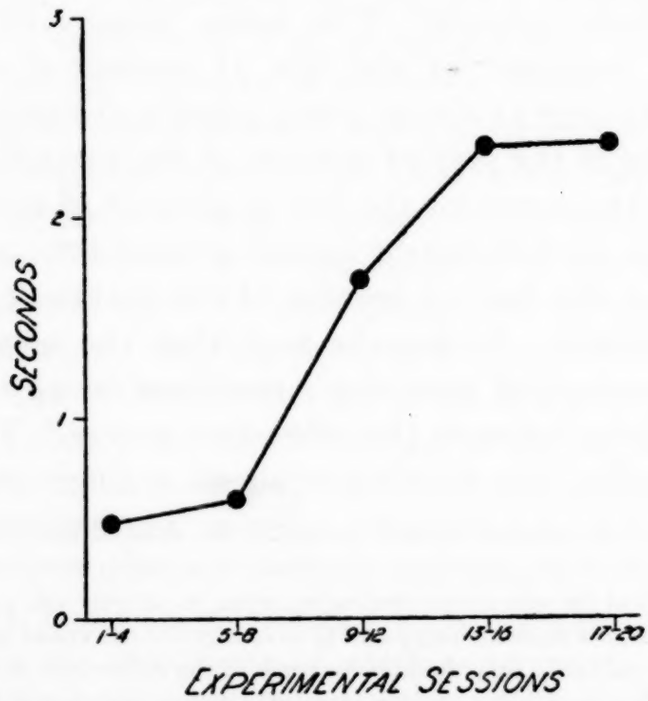


FIG. 5. Curve showing the difference in mean latency of the conditioned galvanic skin response between the first and last 25 percent of each day's trials for successive periods. See Fig. 4.

This shortening of the latency of the trace conditioned reaction between experimental sessions amply supports Switzer's (**21**) finding that a 24-hour interval causes the delayed conditioned response to move forward towards its original position. However, in another study (**22**) he found very little decrease in latency after the delayed response be-

came well established. This was evidently not true in the present case even though the curve of development of delay had begun to flatten out. However, it might have occurred had the training been carried on to a point where no further development of the trace conditioned response ensued.

The fact that this shortening of the latency of the anticipatory response at the beginning of each day still continued after 20 days of reinforcement is of considerable theoretical importance. Pavlov (**18**, 246) regards this as contributing evidence for his theory of the inhibition of delay, since the phenomenon can be explained by the principle of the dissipation of the inhibitory tendencies over a period of time. The fact that this continued over a considerable number of days does indicate that it may be a real phenomenon associated with trace and delayed conditioned reactions.

Another interesting point revealed by Fig. 4 is the initial positive acceleration of the curve of the latencies of the conditioned response for the last 25 percent of each day's trials. Such learning curves with an initial phase of positive acceleration have been reported previously with conditioned responses and complex learning situations (**6**, **8**, **9**, **14**, **23**, **24**, **26**). Hull (**10**) has pointed out that "it seems likely that the S-shaped curve is characteristic of a complete learning process," such as is involved in complex and difficult learning situations.

It was observed that frequently the anticipatory response consisted of a dual wave, with the second rise close to the point of reinforcement. This was similar to the phenomenon noticed with delayed conditioned responses, with the added difference that these secondary reactions showed an ever increasing tendency to merge with the unconditioned response to the shock to form a single wave, so that it was impossible to distinguish the separate reactions. This double response is shown in Fig. 3.

The frequency of these secondary responses is modified by training at a negatively accelerated rate, as shown in the following table:

	Sessions			
	1–5	6–10	11–15	16–20
Frequency in percent.....	16.4	23.0	26.3	28.6

The failure to find this increase in frequency with training in the preceding experiment with delayed conditioned responses may be a direct result of an actual difference between these two types of conditioned responses, although it is quite possible that the much more rapid development of the delayed conditioned response has disguised this trend.

As was observed in the preceding experiment with delayed conditioned responses, there is a clear tendency for the latency of the secondary response to increase in a manner roughly similar to that of the primary conditioned response. The mean latency in seconds for successive groups of five experimental sessions is as follows:

12.36 sec., 13.82 sec., 15.34 sec., and 14.60 sec.

The superimposed secondary reactions do not appear to be a result of an association with some higher symbolic level such as counting, since all of the subjects denied activity of this sort when asked for an introspective report at the conclusion of the experiment.[11]

The latencies of the anticipatory response were plotted, as in Fig. 2, in order to determine whether the primary conditioned response consisted of two types (a short latency response, and a long latency response near the point of reinforcement), as would be the case if the earlier primary

[11] The fairly typical report of one subject is of interest in this connection:

"I do not anticipate or worry about the coming shock as soon as I feel the vibrator. My train of thought or daydream continues for some time after the vibrator has been felt. Then, often without conscious effort or volition on my part, my mind becomes 'blank' and I only wait for the shock. This wait varies considerably in duration and sometimes, if my daydream is particularly pleasant, the shock takes me unawares, without any 'blank' period of waiting."

The onset of this 'blank' period seemed to occur about at the time of the anticipatory response, and appears to have been part of it. This interrelationship of higher symbolic levels of reactivity and lower-order involuntary response systems in conditioning situations may be an interesting problem for future investigators.

conditioned response were extinguished and the secondary reaction were left. If this were true, the distribution should appear as a bimodal curve. That this is not the case may be seen from an inspection of Fig. 6, where the skewed curves, representing each third of the total number of training trials for each subject, show no signs of bimodality. This was supported by an inspection of the individual distributions of

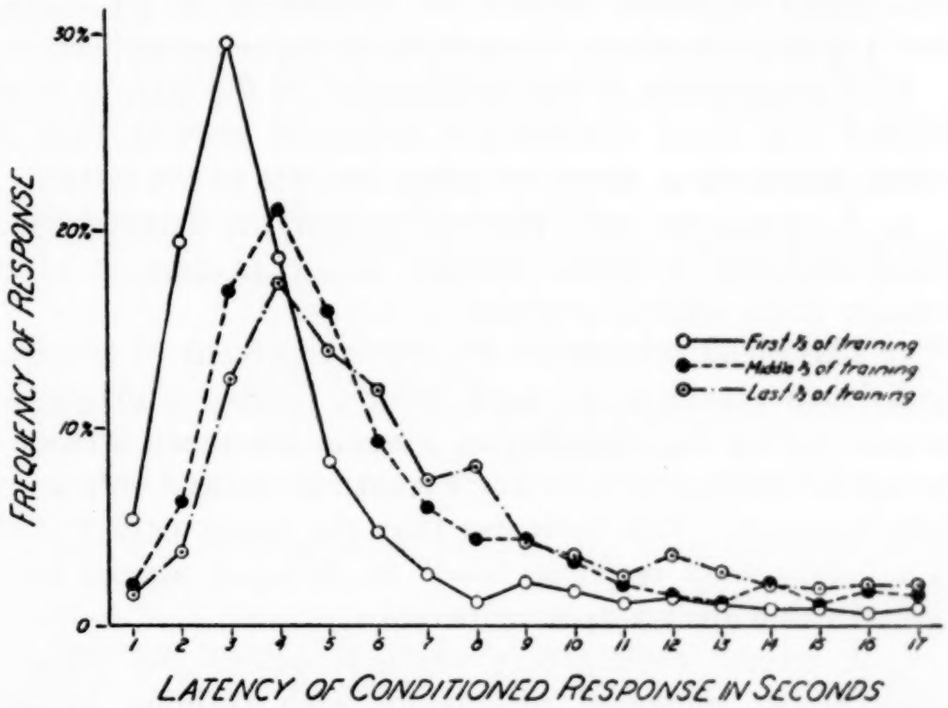

30%
20%
10%
0
FREQUENCY OF RESPONSE
First ⅓ of training
Middle ⅓ of training
Last ⅓ of training
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17
LATENCY OF CONDITIONED RESPONSE IN SECONDS

FIG. 6. Composite graph showing the distribution of latencies of the trace conditioned galvanic skin response for each one-third of the training for 5 subjects. The total number of measures for each successive third of the training trials is 720, 657, and 601 respectively.

the subjects. Thus the indication seems to be that there is a recession of the primary conditioned reaction with no evidence of its replacement by the secondary reactions.

SUMMARY

1. The development of the delayed conditioned galvanic skin response with training was studied in 20 subjects. A faint light of 21.3 seconds' duration served as the conditioned stimulus. An electric shock delivered 20.1 seconds after the onset of the persisting light was used as the reinforcing stimulus. Five other subjects were employed in a second

experiment with trace conditioned responses. In this experiment, a tactual-vibratory stimulus of .180 second's duration was followed after an interval of 17.4 seconds by an electric shock.

2. The mean latency of the delayed conditioned response to the light increased 5.7 seconds during the experimental session of 47 stimulations. The mean latency of the trace conditioned response showed an increment of 4.1 seconds after 374 reinforcements comprising 20 experimental sessions.

3. A comparison of the modification of the latency of the delayed and trace conditioned responses showed that the former increased at about 10 times the rate of the latter.

4. A 15-minute 'rest' interval caused the delayed conditioned response to move forward about 42 percent of the distance to its original position.

5. The mean latency of the last 25 percent of the trace conditioned reactions for each session showed a progressive increase during the experiment, whereas the mean latency of the first 25 percent of each day's reactions showed only a very slight increase. This indicates that the latent period of the trace conditioned response tends to decrease almost to its original value after a 24-hour period.

6. A second anticipatory reaction nearer the point of reinforcement increased in latency with training in both experiments. In the delayed conditioned response experiment, the frequency of this response was fairly constant at about 32 percent of the cases, whereas in the trace conditioned response experiment, this frequency increased at a negatively accelerated rate with continued training.

(Manuscript received September 29, 1936)

References

1. Bass, M. J. and Hull, C. L., The irradiation of a tactile conditioned reflex in man, *J. Comp. Psychol.*, 1934, 17, 47–66.
2. Ebbinghaus, H., *Memory* (Trans. by H. A. Ruger and C. E. Bussenius), New York: Teachers College, Columbia Univ., 1913, pp. xiii + 123.
3. Guthrie, E. R., *The psychology of learning*, New York, Harper and Bros., 1935, pp. viii + 258.
4. Hall, M. E., Remote associative tendencies in serial learning, *J. Exper. Psychol.*, 1928, 11, 65–76.

5. Hilgard, E. R., Conditioned eyelid reactions to a light stimulus based on the reflex to sound, *Psychol. Monog.*, 1931, **41**, No. 184, pp. 50.
6. ——, Modification of reflexes and conditioned reactions, *J. General Psychol.*, **9**, 210–215.
7. Hilgard, E. R. and Marquis, D. G., Acquisition, extinction, and retention of conditioned lid responses to light in dogs, *J. Comp. Psychol.*, 1935, **19**, 29–58.
8. Hilgard, E. R. and Campbell, A. A., The course of acquisition and retention of conditioned eyelid responses in man, *J. Exper. Psychol.*, 1936, **19**, 227–247.
9. Hull, C. L., Quantitative aspects of the evolution of concepts, *Psychol. Monog.*, 1920, **28**, No. 123.
10. ——, Learning: II. The factor of the conditioned reflex, In *Handbook of Experimental Psychology*, edited by C. Murchison, Clark University Press, 1934.
11. ——, The conflicting psychologies of learning, a way out, *Psychol. Rev.*, 1935, **42**, 491–516.
12. ——, The influence of caffeine and other factors on certain phenomena of rote learning, *J. General Psychol.*, 1935, **13**, 249–274.
13. ——, The trace conditioned reflex and serial learning (Unpublished manuscript).
14. Kleitman, N. and Crisler, C., A quantitative study of the salivary conditioned reflex, *Amer. J. Physiol.*, 1927, **79**, 571–614.
15. Leonow, W. A., Über die Bildung von bedingten Spurenreflexen bei Kindern, *Arch. f. d. ges. Physiol.*, **214**, 305–319.
16. Lepley, W. M., A theory of serial learning and forgetting based upon conditioned reflex principles, *Psychol. Rev.*, 1932, **39**, 279–288.
17. ——, Serial reactions considered as conditioned reactions, *Psychol. Monog.*, 1934, No. **205**.
18. Pavlov, I. P., *Conditioned reflexes*, Trans. by G. V. Anrep, London, Oxford Univ. Press, 1927, pp. 430.
19. Rodnick, E. H., Does the interval of delay of conditioned responses possess inhibitory properties? *J. Exper. Psychol.* (in press).
20. Steckle, L. C., A trace conditioning of the galvanic reflex, *J. General Psychol.*, 1933, **9**, 475–480.
21. Switzer, S. A., Anticipatory and inhibitory characteristics of delayed conditioned reactions, *J. Exper. Psychol.*, 1934, **17**, 603–620.
22. ——, The influence of caffeine upon 'inhibition of delay,' *J. Comp. Psychol.*, 1935, **19**, 155–175.
23. Thurstone, L. L., The learning curve equation, *Psychol. Monog.*, **26**, No. 114.
24. Wendt, G. R., An analytical study of the conditioned knee-jerk, *Arch. Psychol.*, 1930, **18**, No. 123.
25. Witmer, L., *Remote association in serial learning*, Ph.D. Thesis, Yale Univ. Library, 1935.
26. Wolfle, D. L., An experimental approach to the study of language. In *Readings in Experimental Psychology*, Ed. by W. L. Valentine, New York, Harpers, 1931, pp. 537–551.

THE EFFECTS OF INACTIVITY PRODUCED BY SODIUM AMYTAL ON THE RETENTION OF THE MAZE HABIT IN THE ALBINO RAT

BY ROGER W. RUSSELL

Clark University

AND

WALTER S. HUNTER

Brown University

Introduction

The object of the present experiments was to determine the effects of inactivity produced by sodium amytal on the retention of the maze habit in the albino rat. Preliminary experimentation on 24 normal animals indicated that where complete learning of the present maze to 3 perfect trials in succession was accomplished in successive trials on one day, retention was practically perfect for most animals after a period not to exceed 48 hours. In order therefore, to show a possible beneficial effect of amytal anesthesia it was necessary to use partial learning, which would carry with it only partial retention. Consequently, in the main investigation rats were given 5 trials on the maze followed by normal activity or anesthesia, and then given the 5-trial retest. The results failed to show a significant difference between the retest records of the normal animals and the comparable records for the anesthetized animals.

Apparatus and Subjects

For preliminary training a straight-away was used in which the starting box and the food box were identical with those of the experimental maze. The straight-away was also similar to the maze in construction, except that there were no turns or blinds. For all the trials with all the animals the straight-away had a length of 68 inches from the exit of the starting box to the entrance of the food box. Retracing doors were hung at intervals along the path and were lowered at random during the preliminary training in order to acquaint the animal with the actual situation to be met in the maze.

The pattern of the maze is shown in Fig. 1. It was assembled from alleys 34 inches long, 4 inches wide, and 4 inches deep, inside dimensions. The top was of wire mesh and the whole was painted black. During the experiment the maze was elevated on supports to a height of 24 inches from the floor of the room. Retracing doors of

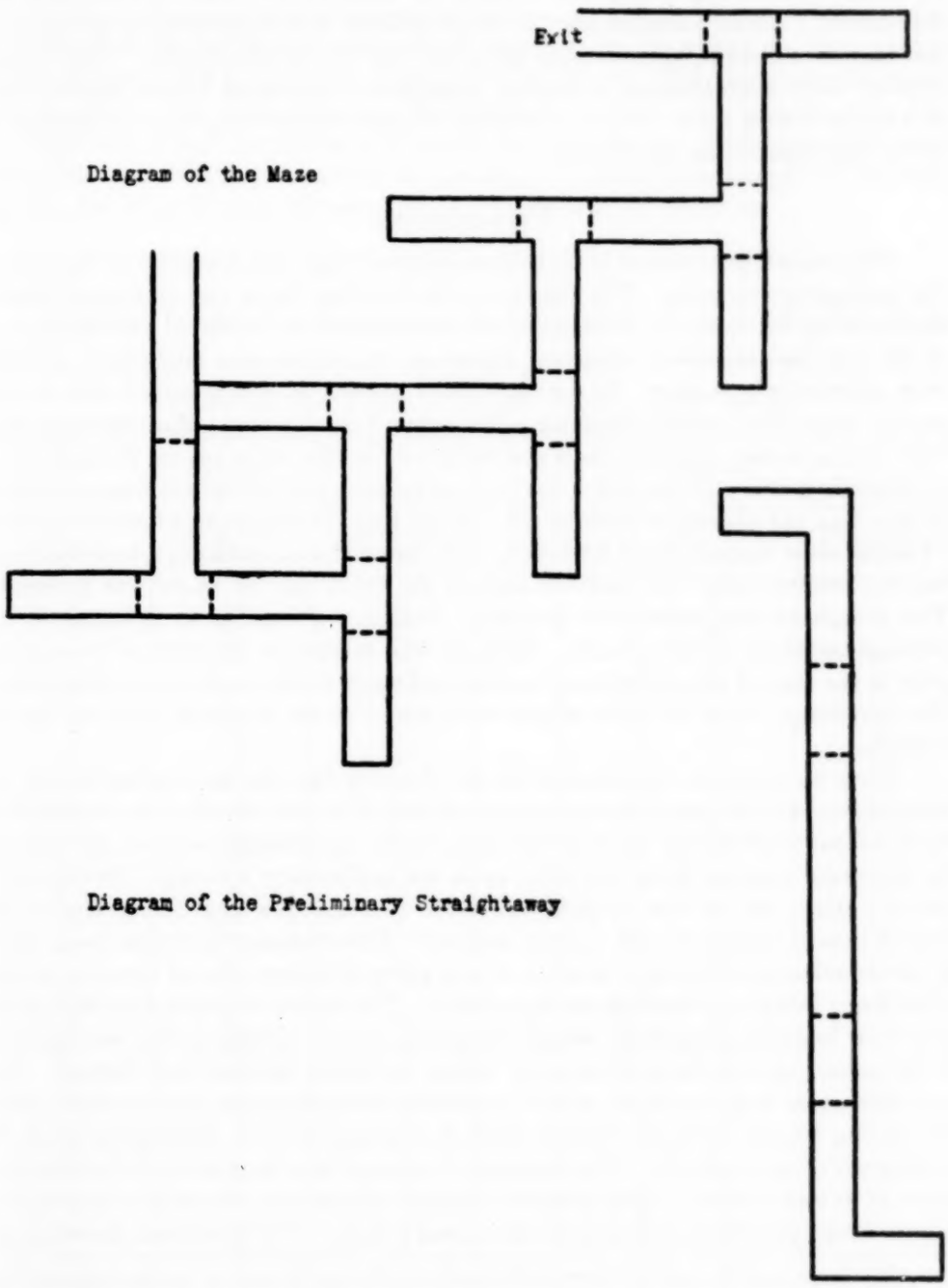


FIG. 1

wire mesh were hung at various points, as indicated in Fig. 1 by the broken lines. The animals were released from the starting box by a vertical sliding door. All the trials, both on the straight-away and on the maze, were given in a room which was approximately sound-proof. For each trial the animal was carried into this room and left

there until the completion of the run. The releasing of the rat and the operation of the retracing doors, as well as the recording of time and errors were accomplished by the experimenter from outside the room.

All of the rats were 5 months old and were male descendants of highly inbred Wistar stock. Variability was further reduced by using as few litters as possible in the experiment. About 2 months after birth the animals were separated into groups of 6, and they then lived in isolated cages up to the time of experimentation. Each animal received daily approximately 2 heaping teaspoons of moistened Purina Rabbit Chow at a single feeding time. Water was before the rats in the living cages constantly up to the beginning of the experiment.

Procedure

The animals were placed in individual isolation cages one day prior to the start of the preliminary training. The following day training began and continued with 3 trials per day for 10 days. Particular care was exercised in control of feeding, because of the fact that otherwise consistent differences in performance might have resulted from differences in hunger. At the end of each run the rat was rewarded with a small portion of dry food (about 1/8 of one pellet) and 0.1 cc. of water. Both amounts were kept approximately constant, the water being fed directly to the animal through a 1 cc. graduated pipette. At the end of the last run for each day the animal was returned to its own cage and allowed to drink its fill. It was then fed one heaping teaspoon of food. This procedure was continued for 8 days. On the ninth and tenth days, however, feeding was omitted after the final run and on the tenth day no water was presented. This completed the preliminary training. Weight was used as an indicator of the physical condition of the animals. Each rat was weighed at the time of its isolation prior to the start of the preliminary training and again on the tenth and eleventh days. The percentage losses in body weight were found to be relatively constant for all animals.

Trials on the maze were started on the eleventh day, the motivation having developed from the two previous days of starvation and the one day of water deprivation. Each animal received five trials on the maze in one experimental session, the rewards for successful running being the same as in the preliminary training. Having completed 5 trials, the rat was weighed and either returned to a large living cage in the animal room or injected with sodium amytal.[1] Subcutaneous injections were made in the dorsal-caudad region, a solution of 0.25 gram of sodium amytal in 2.5 cc. of distilled water being used through the experiment. The female standard dose of 10 milligrams per hundred grams body weight was given, and the administration was repeated if the animal showed signs of recovery before an 8-hour interval had elapsed. The rats were under deep anesthesia within 10 minutes of the injection and remained under for varying lengths of time. They exhibited no excess activity while going under or coming out of the hypnotic. The criterion of recovery was the ability of the animal to stand of its own accord. Upon complete recovery the rat was returned to its isolation cage and remained there until time for the second 5 trials. The latter took place exactly

[1] Nicholas and Barron (3) have calibrated the dosage of sodium amytal against the weight of the albino rat and have observed the effects of different amounts. The doses were based on a solution made by dissolving 1 gram of the salt in 10 cc. of distilled water. It was found that there was a definite sex difference in the reaction to the drug and that the effectiveness of the drug was in direct ratio to its concentration. The safe limit for the injection (standard dose) of the females was found to be 10 milligrams per hundred grams and for the males 20 milligrams per hundred grams of body weight.

24 hours after the completion of the first 5 trials. Table 3 gives the number of hours during which each rat remained under amytal. During the interpolated 24-hour period the control animals lived under their usual conditions in the cages. During the 24 hours between the completion of the first 5 trials and the 5 retest trials neither the normal nor the anesthetized rats received food or water.

The animals were originally paired as closely as possible on the basis of the number of errors made during the first 5 trials on the maze, one of the pair being designated as a control animal and the other as an experimental animal. Only forward-going errors were recorded. The animal was considered to have made an error when it entered by its entire body length (excluding tail length) into a cul-de-sac. Time was noted when the animal passed between two arbitrary points, one 6 inches from the exit of the starting box, the other the same distance from the entrance to the food box.

Results

Table 1 presents a summary of the errors made by each

TABLE 1

Errors on the First Five and Second Five Trials

Normals			Amytals		
Animal	First Five Trials	Second Five Trials	Animal	First Five Trials	Second Five Trials
1	14	2	1	14	5
2	17	11	2	17	2
3	20	6	3	20	10
4	21	12	4	21	11
5	19	6	5	19	4
6	15	5	6	15	7
7	13	6	7	13	0
8	14	2	8	14	2
9	11	1	9	11	0
10	19	4	10	18	3
11	17	7	11	17	5
12	7	1	12	6	0
13	21	4	13	20	6
14	15	0	14	15	2
15	10	2	15	10	2
16	10	4	16	10	0
17	15	4	17	16	6
18	19	4	18	17	4
19	18	7	19	16	7
20	12	0	20	12	0
21	8	1	21	9	0
22	11	0	22	11	0
23	8	5	23	9	0
24	11	3	24	10	1
25	14	9	25	14	1
26	12	0	26	12	5
27	8	0	27	8	0
28	7	1	28	8	0
29	9	0	29	9	0
30	8	0	30	9	0
31	12	1	31	10	3
32	13	3	32	13	3
33	11	0	33	11	4

rat during the experiment. The extent to which the normal and the amytal groups were equated on the basis of initial learning can be seen by comparing columns 2 and 5. Furthermore, the means, medians, and standard errors for the first 5 trials as shown in Table 2, indicate that the distributions are practically identical.

TABLE 2

STATISTICAL MEASURES OF THE ERROR RECORDS ON THE TEST AND RETEST TRIALS

First Five Trials

	Mean	σ_M	Median	$\sigma_{Med.}$
Control animals	13.27	0.73	12.25	0.92
Amytal animals	13.15	0.69	12.25	0.86
Second Five Trials				
Control animals	3.36	0.56	3.25	0.70
Amytal animals	2.82	0.95	2.63	0.21

Critical Ratios for the Differences

Second Five Trials

	D_{1-2}	$\sigma_{diff.}$	$\frac{D}{\sigma_{diff.}}$
Means	0.54	1.10	0.49
Medians	0.62	0.73	0.87

It is possible in the present case to employ two methods for determining whether or not amytal has had an effect on retention. The first method is to determine the reliability of the observed difference between the means and medians for the two groups of retest trials. The second method is to correlate the time under amytal with the amount of retention shown by the rats of the amytal group. Table 2 gives the means, medians, and standard errors for the control and experimental animals, and it also indicates the reliability of the differences between the two groups. It will be seen from these tables that the difference between the control animals and the experimental animals is not statistically significant,

either in the case of the means or of the medians, although in both cases the actual figures favor the experimental animals.

Figures 2 and 3 show the distribution of error scores for the two groups of animals on the first 5 trials and the second 5 trials respectively. The distributions in Fig. 2 approximate the normal curve, whereas those of Fig. 3 are so badly skewed that the use of the standard statistical measures is hardly justified. The small number of cases and the skewness of the

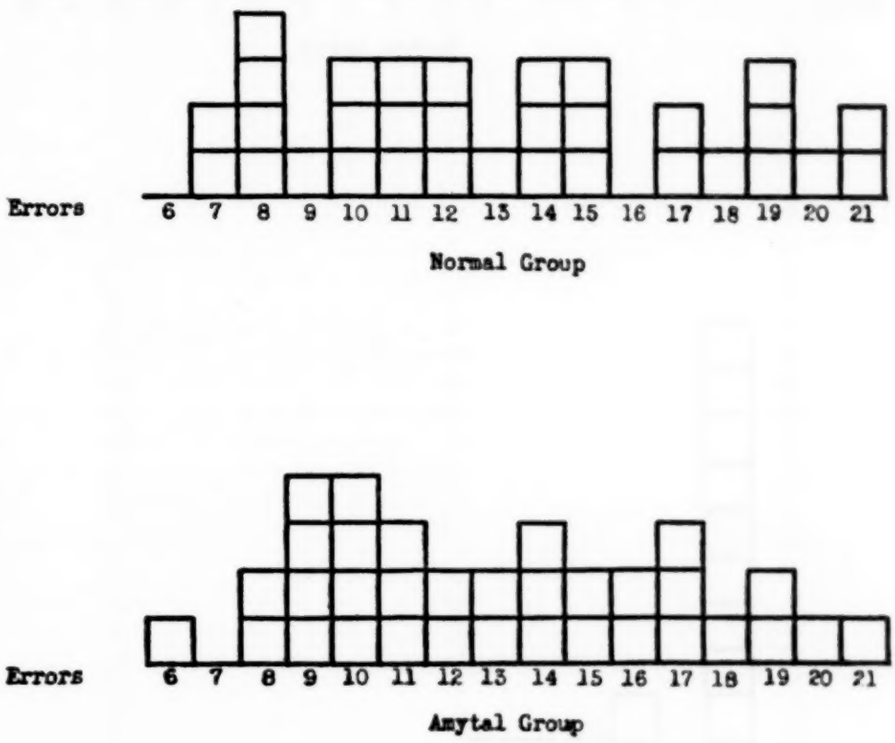
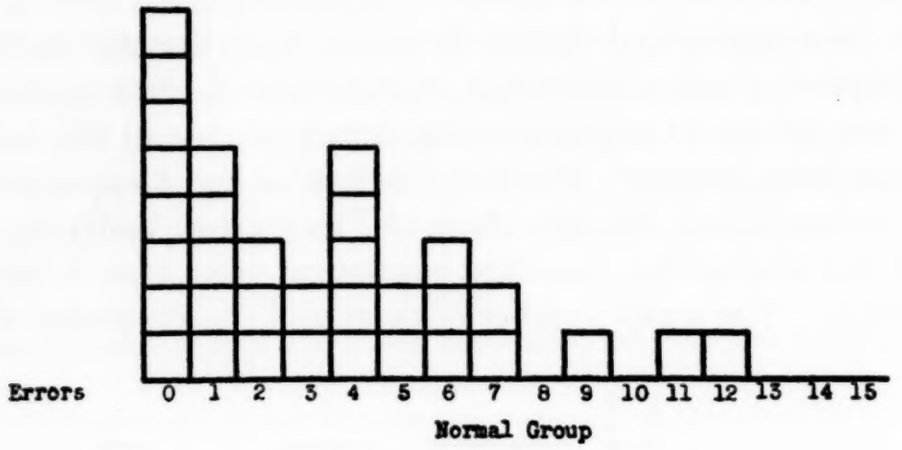


FIG. 2. First five trials.

distribution would indicate that the median is the better measure of central tendency, and it is interesting to note in Table 2 that the difference between the median error scores of the two groups of rats is more nearly significant than the difference between the means. Perhaps a more dependable method of comparing the two groups would be to point out that 12 of the amytal rats (36 percent of the group) made no errors on the second 5 trials, that is, showed perfect retention, whereas only 8 of the normal animals (24 percent of the group) made no errors on the second 5 trials.



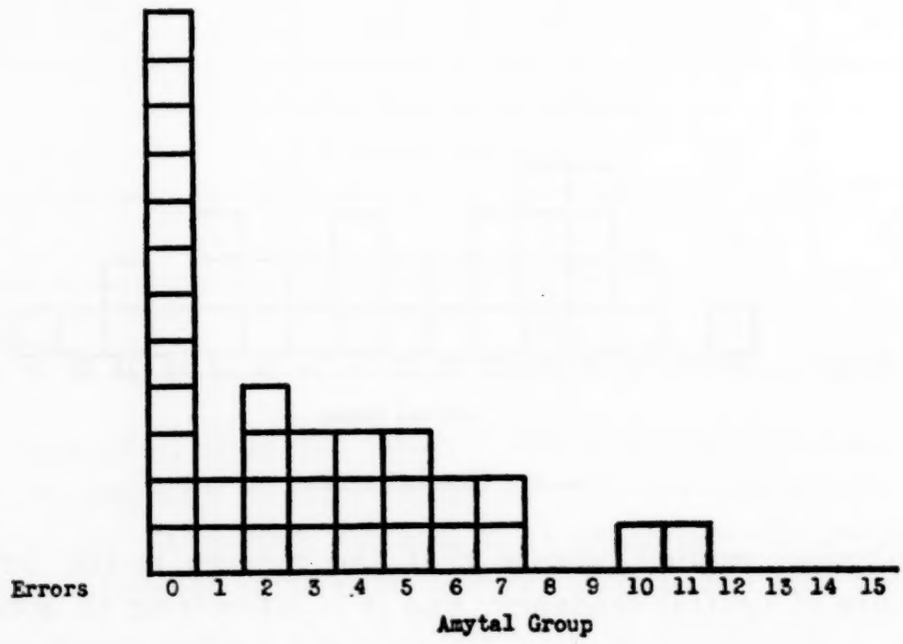


FIG. 3. Second five trials.

Table 3 shows the number of hours during which each animal was under amytal anesthesia and also the amount of retention of the effects of maze training, where this is determined by subtracting the error score on the second 5 trials from the error score on the first 5 trials for each animal. The correlation between the time under amytal and the amount of

retention is $+0.084 \pm 0.12$. There is thus no indication within the amytal group of a relationship between retention and the duration of anesthesia.

TABLE 3

RETENTION AND TIME UNDER AMYTAL

Animal	Retention	Hours under Amytal
1	9	14
2	15	12
3	10	11
4	10	10
5	15	11
6	8	13
7	13	11
8	12	10
9	11	10
10	15	17
11	12	8
12	6	12
13	14	9
14	13	12
15	8	8
16	10	10
17	10	13
18	13	10
19	9	11
20	12	8
21	9	10
22	11	14
23	9	10
24	9	12
25	13	8
26	7	9
27	8	14
28	8	8
29	9	9
30	9	10
31	7	9
32	10	11
33	7	11

Since no significant effect of the amytal anesthesia is revealed by the above data,[2] the question arises as to whether or not the amytal animals were physiologically handicapped by the drug and so were prevented from manifesting a retention superior to that of the normal animals. Since 24 hours elapsed between the two groups of 5 trials on the maze, an

[2] G. W. Williams (4) reports the use of sodium phenobarbital in a study of learning and retention in the rat. The results indicate that the anesthetized animals were inferior to the normals in learning and relearning but that the two groups were equal in retention.

inspection of Table 3 will show that no period of recovery from the drug was less than 7 hours, whereas the period of recovery might be as long as 16 hours. There seems little reason to believe, therefore (so far as this evidence can indicate), that an inadequate recovery time was allowed. Second, a comparison of the time records for the normal and amytal rats on the 5 retest trials (Fig. 4) indicates that the two groups

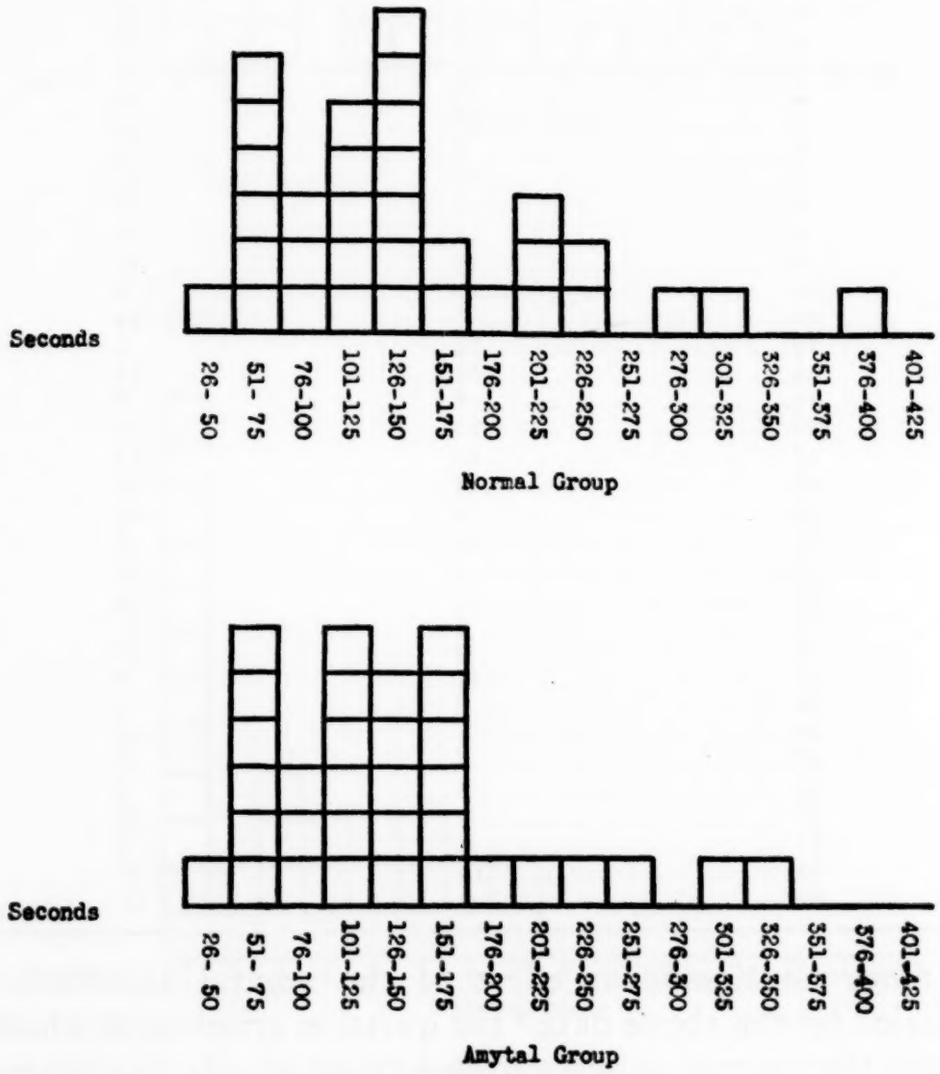


FIG. 4. Second five trials.

of animals ran equally rapidly, so that in so far as running time in the maze is an indication of physiological fitness the amytal rats were not handicapped. Third, there is the possibility that administration of amytal altered the drive of the rats and thereby affected the relearning records of the

amytal group. In order to check this possibility a separate experiment was performed on 6 new male rats, each of which was first tested as an amytal and then as a normal animal. The rats were first isolated and fed precisely as the experimental amytal animals of the main experiment above recorded. They were therefore deprived of food for 2 days and of water for one day prior to the test now being described. The animals were then anesthetized with a single injection of the standard female dose and 24 hours later, during which time no water was given, were tested on water consumption. The animal, while still in its isolation cage and unhandled, was offered water for a period of 5 minutes from a graduated pipette. The animals were allowed to drink as rapidly as they wished, and it was found that no animal drank constantly for the entire test period. For one week following this test the rat was well fed and watered, after which it was again deprived of food and water for 72 hours and 48 hours respectively before being tested for normal water consumption. The results in Table 4 show that 5 of the 6 animals drank less

TABLE 4

EXPERIMENTAL WATER CONSUMPTION OF RATS UNDER NORMAL AND POST-AMYTAL CONDITIONS

Rat	Weight, gm.	Water Consumption in cc.	
		Post-amytal	Normal
1	197	3.86	5.96
2	202	7.47	9.93
3	205	5.40	8.31
4	207	4.65	9.97
5	205	7.77	6.91
6	215	3.80	5.93
		Ave. 5.47	Ave. 7.83

water in the post-amytal test than in their normal test and that one animal drank slightly more in the post-amytal than in the normal test. To the extent that these results are representative and to the extent that amount of water consumption indicates strength of thirst drive, we would conclude that the normal animals may have had a slightly stronger

thirst drive than the amytal animals. It is possible, therefore, in spite of the data on speed of running above given, that the amytal animals were still physiologically affected by their anesthesia.

Conclusions

There is no clear evidence that the retention of a partially learned maze habit is better in rats that have been anesthetized with sodium amytal for from 8–17 hours of an interpolated 24 hours than it is in normal rats after a 24-hour period. This conclusion is supported by the essential statistical equality of the mean and median error records of the two groups. Furthermore, the correlation between length of time under amytal and retention scores is negligible. The evidence indicating a possible favorable effect of the anesthesia on retention is: (1) The number of amytal rats who showed perfect retention was 50 percent higher than the number of normal rats making such a record; and (2) there is some evidence that the thirst drive in the amytal rats was less than in the normals.

(Manuscript received October 27, 1937)

References

1. Hunter, W. S., The effect of inactivity produced by cold upon learning and retention in the cockroach, Blatella germanica, *J. Genet. Psychol.*, 1932, **41**, 253–266.
2. Jenkins, J. G., and Dallenbach, K. M., Obliviscence during sleep and waking, *Amer. J. Psychol.*, 1924, **25**, 605–612.
3. Nicholas, J. S., and Barron, D. H., The use of sodium amytal in the production of anaesthesia in the rat, *J. Pharmacol. & Exper. Ther.*, 1932, **46**, 125–129.
4. Williams, G. W., The effect of sodium phenobarbital on the learning of rats, *Psychol. Bull.*, 1936, **33**, 745–746.

BLOCKING IN MENTAL FATIGUE AND ANOXEMIA COMPARED

BY ARTHUR G. BILLS[1, 2]

University of Chicago

Previously reported studies (**2**, **3**, **5**) have shown that the most sensitive indicator of mental fatigue is the increase in frequency and length of the involuntary pauses or 'blocks' which occur more or less periodically in mental work. That these blocks have a recuperative function, giving the response mechanisms frequent rests, is shown by the fact that the response times do not lengthen appreciably, nor is there a decided increase in errors with prolonged work. The neurophysiological correlate of this picture would be that a gradual depletion of the available neural energy necessitated frequent forced pauses.

It is known that *active* neural tissue undergoes more rapid metabolism and hence more rapid consumption of available oxygen takes place. Does this reduction of available oxygen in neural or muscular tissue explain the increase in length and frequency of the block pauses? The problem can be approached indirectly by noting the effect of an artificial reduction in the percentage of oxygen in the inspired air upon the blocking tendency of subjects performing mental work. If the same change in frequency and length of blocks occurs as results from continuous work for a prolonged period, say an hour, then we have presumptive, though by no means conclusive, evidence that the oxygen depletion factor is one of the factors involved in producing the symptoms of mental fatigue.

It was the purpose of the present investigation, therefore, to secure answers to the following questions:

[1] The present investigation was aided in part by a grant to the University of Chicago from the Rockefeller Foundation.

[2] The entire apparatus for control of ventilation was designed and constructed by my research assistant, Mr. Edward Geldreich, and entire credit for it should go to him.

(1) To what extent are the changes in mental performance brought about by anoxemia comparable to those from prolonged work? More specifically, is there a comparable increase in length and frequency of blocks without an appreciable slowing up in reaction time?

(2) Do the changes characteristic of fatigue from mental work performed under normal air develop more rapidly under anoxemia conditions?

Summary of Related Literature

The evidence that mental work involves increased oxygen consumption comes from several sources but is not unequivocal. On the one hand, there is the evidence showing that there is increased metabolism in nervous tissue when impulses are conducted, involving production of CO_2, etc. Gerard has summarized this evidence (**6**). It is probable that the rate of metabolism in cortical tissue is much higher than in peripheral nerve. Then, again, there is much evidence of muscular involvement in mental work, and working muscle consumes oxygen rapidly. On the other hand, there is the evidence from studies of respiratory calorimetry of the body as a whole, and from chemical analysis of the blood and urine, during and after mental work. Recent studies report increases in metabolic rate all the way from zero up to 28 percent and more, above the resting rate (**9**). A good review of this literature is contained in the report of Benedict and Benedict (**1**). Obviously any changes due to activity of the brain alone would be considerably diluted in the metabolic products of the whole organism.

Blood analyses have been made for the body as a whole, and for the brain alone by internal jugular vein and carotid artery punctures (**7**). The results are equivocal, due to technical difficulties, but do not offer evidence contradictory to the general theory.

Several studies have dealt with the general effects of anoxemia on mental performance. Two methods have been employed: one is the rebreathing method which gradually reduces the oxygen content of a limited amount of air, by

rebreathing, until the subject is no longer able to carry on. It has the disadvantage of not enabling the experimenter to determine the effects of given *constant* amounts of oxygen. The other method is to obtain a large supply of air of a given oxygen content by mixing air with nitrogen in a container beforehand, and then to allow the subject to breathe this constant mixture while performing work. This method was used in a recent study by Ross McFarland (**8**). It is the method which was employed in the present investigation. McFarland used about the same range of oxygen percentages, and studied, among other things, discreet reaction times to color stimuli. His results will be discussed in connection with those of the present study, because of the bearing they have upon it. McFarland gives an excellent historical review of the subject of anoxemia.

Apparatus and Procedure.—For purposes of description, the apparatus can be divided into two parts, (1) the breathing apparatus, and (2) the apparatus for presenting stimuli and for receiving and recording the subjects' responses.

(1) *Breathing Apparatus.*—Normal air contains 20.9 percent oxygen. In order to reduce the oxygen percentage in the inspired air by a definite amount, it is necessary to dilute normal air by adding nitrogen to it in the proper proportions. If the subject is to breathe air of a certain reduced oxygen content for a considerable period of time, such as an hour, it is necessary to prepare a quantity of such a mixture in an appropriate container beforehand, so that the exact oxygen percentage can be tested by chemical analysis of a sample.

The container for the gas mixture was constructed of rubberized cloth with a capacity of 45 cubic feet. Being collapsible when deflated, it had to be mounted within a framework of metal rods, by means of sliding rings. The air was pumped into the bag by means of a converted vacuum cleaner, and the nitrogen was introduced from compression tanks. Before entering the bag, the gasses were piped through a gas flow meter, so that an approximate estimate could be made of the volume of each gas introduced. Chemical analysis showed that the estimates based on the meter reading were never more than two tenths of a percent off. When the mixture had been prepared, a sample was then taken and analyzed by means of an Orsat-Lunge chemical gas analyzer.

The subject was equipped with a Douglas double valve mouth piece and a nose clip, so that he inhaled through the pipe leading from the gas bag and exhaled into the room, so that his expired air in no way affected the content of the mixture that he breathed. The part of the inhale pipe near the mouthpiece was equipped with corrugated rubber walls to prevent pinching off the air supply.

(2) *The Response and Recording Apparatus.*—The subject sat before the apparatus designed to expose the color stimuli one at a time through a small window. He responded by pressing keys with the right hand corresponding to the different color stimuli. As soon as he responded to a stimulus, the next one was automatically exposed, so that he could work as fast as he was able. The responses were recorded on

the modified typewriter carriage and, at the same time, an electric signal marker recorded the reactions on a kymographic tape, so that each individual reaction time could be later computed. A description of this entire apparatus has already been published (4).

Subjects.—Ten subjects, five men and five women, all graduate students in psychology, took part in this experiment. Not all of them took all the different conditions of oxygen reduction, due to the fact that we were unwilling to run the risk of subjecting the women to the lowest percentages because of danger of fainting and collapse. However, for the purposes of the experiment, it was not really necessary to use oxygen percentages below 10 percent, which is sufficiently above the lower limit of tolerance for most persons. Three subjects took 9 percent and one subject took 8 percent oxygen.

Conditions and Procedure.—Each subject was first put through a series of four practice trials, of a half hour each, on four separate days, in which he breathed normal air from the bag and responded manually to the colors. This practice series was designed (1) to accustom the subject to the breathing apparatus, mouthpiece, etc., and (2) to give him training in responding manually to the keyboard, until he had improved to the point where further improvement from practice would be negligible during the actual experimental sessions. Records of each practice day were kept, and graphs plotted of the improvement, but these are not included here.

Since the main purpose of the experiment was to obtain a comparative picture of performance under a condition of anoxemia and under a condition of fatigue from an hour's continuous work under normal air, it was necessary to get performance records at various levels of oxygen reduction. It was also necessary to run a control condition before and after the experimental conditions were run in order to compute the loss in performance level caused by the anoxemia. Finally, it was necessary to obtain an hour's continuous work record under normal air conditions to bring out the effects of fatigue. Below is given a chart of the conditions of the regular experiment. Every subject took the conditions in a different order, on separate days, except that the two control conditions were always run on the day before and the day after the anoxemia conditions.

The general directions given to the subjects were to work as rapidly at all times as was consistent with a reasonable degree of accuracy. It is always difficult to maintain a constant motivation in mental work. But the maturity and sophistication of these subjects, together with their evident desire to obtain true results, were some guarantee that they maintained performance at their best level. Errors were not excessive. Introspections indicated some inconvenience experienced from the mouthpiece, but a tendency to become adapted with time. As far as possible, the subjects were kept ignorant of what sort of mixture they were breathing. That this ignorance was successfully maintained is shown by the fact that some of them later reported a belief that the control condition was an anoxemia condition, and that some of the anoxemia conditions were controls.

Results of the Experiment

Due to the wide individual differences obtained under the experimental conditions of this study, a mere presentation of the group averages is insufficient to give a true picture. Because of the counter balanced order of taking the different conditions, however, the relative practice advantage for

conditions falling late in the series was only eliminated in the group averages. Consequently, when the results are viewed individually, some subjects actually show better performance under certain percentages of anoxemia than under normal air. It is even possible that the condition of 15 percent oxygen is slightly superior to the normal for many persons. If so, it may offer a possible clue to the phenomenon of 'warming up,' or the tendency for performance to improve with small amounts of 'physiological' fatigue.

CONDITIONS OF THE EXPERIMENT

Condition	First Half Hour	Last Fifteen Minutes
Control I	Subject breathes normal air (20. 9 percent oxygen) while sitting at rest	Subject continues to breathe normal air. Works at color naming continuously.
Experimental I	Breathes 15 percent oxygen. Rests	Breathes 15 percent oxygen. Works at color naming.
Experimental II	Breathes 12 percent oxygen. Rests	Breathes 12 percent oxygen. Works at color naming.
Experimental III	Breathes 10.5 percent oxygen. Rests	Breathes 10.5 percent oxygen. Works at color naming.
* Experimental IV	(Breathes 9 percent oxygen.) Rests	(Breathes 9 percent oxygen. Works at color naming.)
† Experimental V	(Breathes 8 percent oxygen.) Rests	(Breathes 8 percent oxygen. Works at color naming.)
Control II	Breathes normal air. Rests	Breathes normal air. Works at color naming.

Hour Work Record: Subject works for an hour continuously while breathing normal air.

* Only run by three subjects.
† Only run by one subject.

The purpose of the experiment being to compare behavior under anoxemia with that under slight mental fatigue, it was unnecessary to test all the subjects at the lower limits of tolerance. Consequently, most of the extreme symptoms reported by previous investigators, such as acute impairment of muscular control, violent emotional disturbance, and loss of orientation and fainting were only encountered in two subjects, who were given the lowest percentages. Muscular rigidity of the fingers interfered seriously with hitting the keys in the case of subjects G, E and B. Tremors of hand

and head became marked toward the end of the period with subjects G and B, and both of them experienced loss of orientation and loss of sense of time; although neither actually fainted. No emotional disturbance other than an increased sense of the ludicrous was reported—no desire to destroy the apparatus, or fight the experimenter. There was a tendency to perseverate—to plug on doggedly even when told to stop—as though it was easier to continue than to stop. There was also experienced a narrowing of the field of attention and a lack of susceptibility to distracting stimuli. Periods of amnesia were interspersed with periods of clarity.

Methods of Analysis of Results

The results for each subject were analyzed, for each of the experimental conditions, to determine the following values:

1. The average number of responses per minute.
2. The average number of blocks per minute.
3. The total block time per minute (measured in millimeters).
4. The average block length (in millimeters).
5. The average corrected response time.
6. The total block time per minute (measured in reaction times).

The first of these measures is self explanatory. The value represents an average of 15 minutes. The second was obtained by counting the number of blocks (delayed responses) in each minute, and averaging for the fifteen minutes. The third score was obtained by actually measuring every block with a millimeter rule and then averaging. The fourth score is easily computed by dividing the third score (total block time per minute) by the second score (blocks per minute). The fifth score was computed in the following way: Since part of the time in each minute was consumed in blocking, it is necessary to subtract the total block time in millimeters from the total time in the minute in order to determine how much time was actually spent in responding. When this value is determined, then it is divided by the number of responses per minute, to get the *average response time* (exclusive of blocks). The formula would be

$$\frac{177 \text{ millimeters—total block time in millimeters}}{\text{Responses per minute}}.$$

The resulting value is the length of the average response (in millimeters).

The sixth score was computed by dividing the third score by the fifth score. It is the most significant index of the size of blocks, because it takes into consideration the fact that the length of the blocks at any given time is a function of the rate at which the subject is responding at that time.

Tables I to IV show the results for each of the ten subjects

TABLE I

INDIVIDUAL AVERAGE SCORES FOR CONTROL CONDITION

Subj.	Responses per Min.	Blocks per Min.	Avr. Block Length in Millimeters	Avr. Corrected Response Time in Millimeters	Total Block Time in Responses
D	98.2	7.9	3.4	1.53	17.5
E	73.0	6.6	5.6	1.91	19.35
G	95.8	2.4	4.1	1.74	5.65
K	86.9	5.0	4.4	1.78	12.34
N	101.5	7.3	2.6	1.56	10.5
O	84.1	2.2	4.4	1.99	4.87
P	83.8	2.8	4.3	1.97	6.12
B	68.0	4.2	6.6	2.19	12.63
C	78.6	5.5	3.6	2.13	9.31
M	85.8	1.4	4.9	1.98	3.46
Avr.	85.6	4.5	4.4	1.88	10.17

TABLE II

INDIVIDUAL AVERAGE SCORES EXPERIMENTAL CONDITION I
(15 PERCENT OXYGEN)

Subj.	Responses per Min.	Blocks per Min.	Avr. Block Length in Millimeters	Avr. Corrected Response Time in Millimeters	Total Block Time in Responses
D	99.4	8.3	3.0	1.53	16.3
E	82.4	2.9	4.4	2.00	6.4
G	79.4	5.7	5.7	1.82	17.8
K	87.0	4.1	4.4	1.83	9.9
N	101.9	7.5	2.5	1.56	12.0
O	64.7	1.7	4.6	2.60	3.0
P	81.4	2.5	4.4	2.00	5.5
B	No record				
C	85.4	5.9	3.6	1.84	11.6
M	83.7	3.9	4.8	1.76	10.6
Avr.	85.0	4.72	4.1	1.88	10.3

TABLE III

INDIVIDUAL AVERAGE SCORES EXPERIMENTAL CONDITION II
(12 PERCENT OXYGEN)

Subj.	Responses per Min.	Blocks per Min.	Avr. Block Length in Millimeters	Avr. Corrected Response Time in Millimeters	Total Block Time in Responses
D	96.5	6.6	3.3	1.60	13.6
E	80.7	6.2	5.5	1.77	19.3
G	84.2	6.4	5.4	1.69	20.4
K	91.8	6.3	4.6	1.61	18.0
N	103.0	10.7	2.2	1.49	15.8
O	64.0	0.9	5.7	2.69	1.9
P	82.0	3.3	4.5	1.98	7.6
B	49.0	6.0	8.3	2.59	19.2
C	89.3	6.1	3.4	1.75	11.9
M	82.4	3.3	4.4	1.97	7.4
Avr.	82.3	5.9	4.8	1.91	13.5

TABLE IV

INDIVIDUAL AVERAGE SCORES EXPERIMENTAL CONDITION III
(10.5 PERCENT OXYGEN)

Subj.	Responses per Min.	Blocks per Min.	Avr. Block Length in Millimeters	Avr. Corrected Response Time in Millimeters	Total Block Time in Responses
D	108.5	5.5	3.3	1.46	12.40
E	80.9	8.4	5.1	1.66	25.83
G	64.8	9.3	6.1	1.86	30.57
K	84.2	6.7	4.5	1.74	17.29
N	No record				
O	63.6	3.0	5.6	2.52	6.67
P	72.9	6.1	4.5	2.05	13.39
B	40.0	8.0	8.4	2.74	24.51
C	75.5	5.2	3.9	2.08	9.77
M	85.3	5.3	5.2	1.75	15.74
Avr.	79.0	6.4	5.1	1.98	17.35

for the three experimental and an average of the two control conditions. It can be seen, from an examination of Table I that there are wide individual differences in reaction time, in frequency of blocks and in length of blocks. The correlation between responses per minute and blocks per minute is + .20, indicating no interrelationship, while that between responses and length of blocks is − .52, indicating that the slow workers have the longer blocks. This last coefficient is

increased to — .62, when corrected reaction times are correlated with length of blocks.

A comparison of the records of given individuals under the different conditions, as brought out in Tables I to IV, indicates that, due to the scrambled order in which the conditions were taken, causing the greatest advantage of practice to fall now on one, now on another condition, some subjects actually worked faster and had fewer blocks under anoxemia than under normal air.

But the averages indicate a definite trend toward an increase in blocking which is roughly proportional to the reduction in oxygen below the 15 percent level. Table VIII shows how much more rapid this increase becomes for the lower oxygen levels. Number and length of blocks at the 8 percent level are nearly double the amounts under normal air. Blocks per minute have increased from 4.5 to 8.6. Block length has grown from 4.4 to 7.6 millimeters. Total block time, as measured in reaction times, has multiplied three times from 10.2 to 33.7. Yet the corrected modal reaction time of the subjects has not changed materially. It is 1.88 millimeters under normal air, and 1.94 millimeters under 8 percent oxygen.

This last finding is extremely important because it indicates that anoxemia does not cause a lengthening of the basic reaction time of the subject, but rather it increases his blocking. It indicates that the blocking phenomenon has a very intimate relation to the oxygen metabolism.

As far as the results on the four subjects who were tested show, pure oxygen, when breathed instead of air, during the fifteen minutes of work, does not alter the performance level in any way from the normal. The average scores of Table VII show no significant differences from that of Table I. With a much larger number of subjects, it might be found that the very slight improvement under pure oxygen would turn out to be statistically significant.

Table V gives the individual results of the separate test which was run on each subject to determine the fatigue effects of an hour's continuous work under normal air. The amount

TABLE V

INDIVIDUAL AVERAGE SCORES FOR HOUR WORK PERIOD UNDER NORMAL AIR
(First 5 Minutes and Last 5 Minutes Compared)

Subj.	Responses per Minute		Blocks per Min.		Avr. Block Length in Millimeters		Avr. Corrected Response Time in Millimeters		Total Block Time in Responses	
	1st 5	Last 5	1st 5	Last 5	1st 5	Last 5	1st 5	Last 5	1st 5	Last 5
D	105.8	120.4	6.0	5.0	3.4	3.0	1.50	1.35	13.6	11.1
E	74.0	66.6	4.4	7.5	5.8	5.4	2.0	2.0	12.7	20.2
G	95.8	84.6	2.4	4.0	4.1	5.6	1.74	1.8	5.7	12.4
K	95.2	93.0	4.4	5.4	4.4	4.4	1.65	1.65	12.1	14.4
N	Record lost									
O	75.8	69.8	1.0	5.4	3.0	6.6	2.2	2.0	1.4	18.0
P	89.8	81.0	3.4	4.4	3.3	3.6	1.84	2.0	6.1	8.0
B	73.0	58.2	4.0	9.2	5.9	7.0	2.1	1.93	11.2	33.3
C	88.8	88.4	5.2	8.2	3.4	4.2	1.8	1.6	9.9	21.4
M	Record lost									
Avr.	87.3	82.8	3.8	6.2	4.1	5.0	1.86	1.79	9.1	17.3

TABLE VI

INDIVIDUAL AVERAGE SCORES UNDER 9 PERCENT OXYGEN

Subj.	Responses per Min.	Blocks per Min.	Avr. Block Length in Millimeters	Avr. Corrected Response Time in Millimeters	Total Block Time in Responses
E	73.2	8.3	6.4	1.69	31.44
O	58.1	4.5	6.6	2.54	11.69
B	34.0	9.8	9.9	2.35	41.28
Avr.	55.1	7.5	7.6	2.19	28.13

TABLE VII

INDIVIDUAL AVERAGE SCORES UNDER 100 PERCENT OXYGEN

Subj.	Responses per Min.	Blocks per Min.	Avr. Block Length in Millimeters	Avr. Corrected Response Time in Millimeters	Total Block Time in Responses
B	73.7	4.8	4.9	2.08	11.31
S	99.0	4.0	4.1	1.62	10.12
G	96.0	2.6	4.2	1.73	6.31
E	90.6	6.4	5.4	1.57	22.01
Avr.	89.8	4.45	4.65	1.75	12.44

of decrement is brought out by comparing the performance level for the first five minutes with that for the last five. Since this test was given subsequent to the other conditions of the experiment, the initial performance level shows the advantage of the greater amount of practice as compared with that in the control condition shown in Table I which was also run in normal air.

When we compare the last five-minute level with the first five-minute level, we see that only one subject, subject D, showed improvement. (Two of the records were destroyed by the action of sunlight before the tabulations had been made from them.) All the others suffered a decrement, indicated by increased length and frequency of blocks. Responses per minute also declined, but the corrected reaction time of the subjects underwent no change. This only confirms what has been previously pointed out; namely, that the basic reaction time is not lengthened by fatigue, but rather there is an increase in blocking, which furnishes sufficient rests to compensate for the energy depletion, and enables the organism to continue responding at the original rate.

Comparison of Fatigue and Anoxemia

In Table VIII, a comparison can be made between the

TABLE VIII

Average Group Results in Anoxemia Experiment (All Conditions)

	Responses per Min.	Blocks per Min.	Avr. Block Length in Millimeters	Avr. Corrected Response Time in Millimeters	Total Block Time in Responses
Average normal air.....	85.6	4.5	4.4	1.88	10.17
15 Percent oxygen.....	85.0	4.72	4.1	1.88	10.3
12 Percent oxygen.....	82.3	5.9	4.8	1.91	13.5
10.5 Percent oxygen.....	79.0	6.4	5.1	1.98	17.35
9 Percent oxygen.....	55.1	7.5	7.6	2.19	28.13
8 Percent oxygen.....	57.4	8.6	7.6	1.94	33.69
100 Percent oxygen.....	89.8	4.45	4.65	1.75	12.44
Normal Hour Work Record					
First 5 min.....	87.3	3.8	4.1	1.86	9.1
Last 5 min.....	82.8	6.2	5.0	1.79	17.3

amount of increase in blocking which results from an hour's mental work and the amount resulting from reduction of the oxygen content of the inspired air by various amounts. It can be seen that *the fatigue from an hour's work produces about the same increase in frequency and length of blocks as is produced by reducing the oxygen content to 10.5 percent, or about half.* The blocks increase in frequency from 3.8 to 6.2 per minute and in length from 4.1 to 5.0 millimeters. The total block time (measured in responses) increases from 9.1 to 17.3 per minute. We must be cautious in interpreting this result however. It cannot be assumed that the mental work has

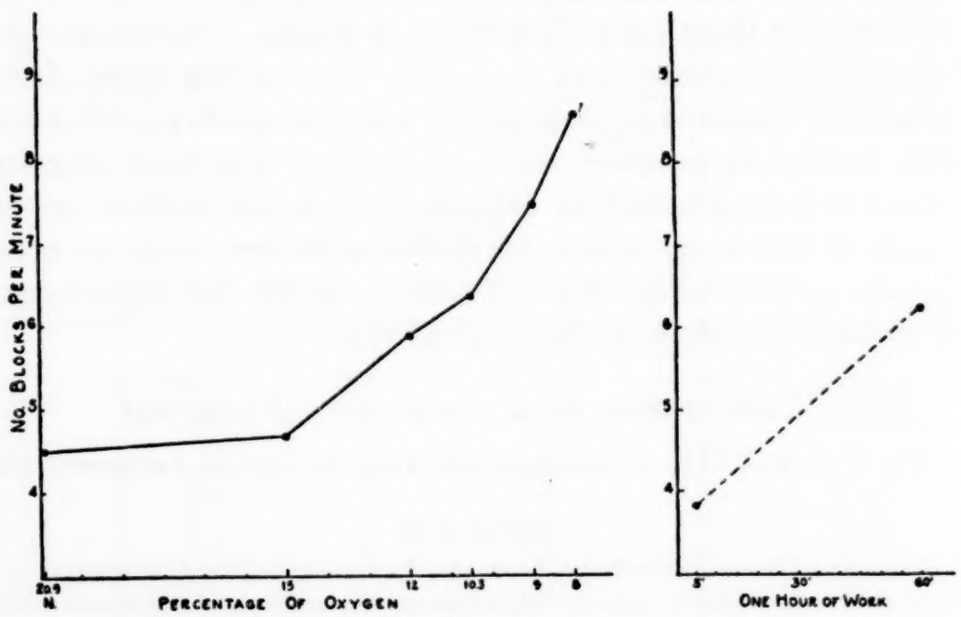


FIG. 1.

actually reduced the available oxygen supply to this extent. There are many other physiological changes wrought by mental work than reducing the oxygen supply. Also, it is not so much the depletion in total supply of oxygen, as in the amount immediately available to the tissues involved, which causes the reduction in efficiency of mental performance.

The results have been expressed graphically for comparative purposes in Figs. 1 to 4. The graph to the left, in each case, represents the effect of reducing the oxygen supply while that to the right represents the effect of an hour's mental work. The general contour of the oxygen curves, in

Figs. 1 to 3, indicates a positively accelerated relationship between the increase in the blocking tendency and the degree of anoxemia. The rise in the curve is steepest for the lowest

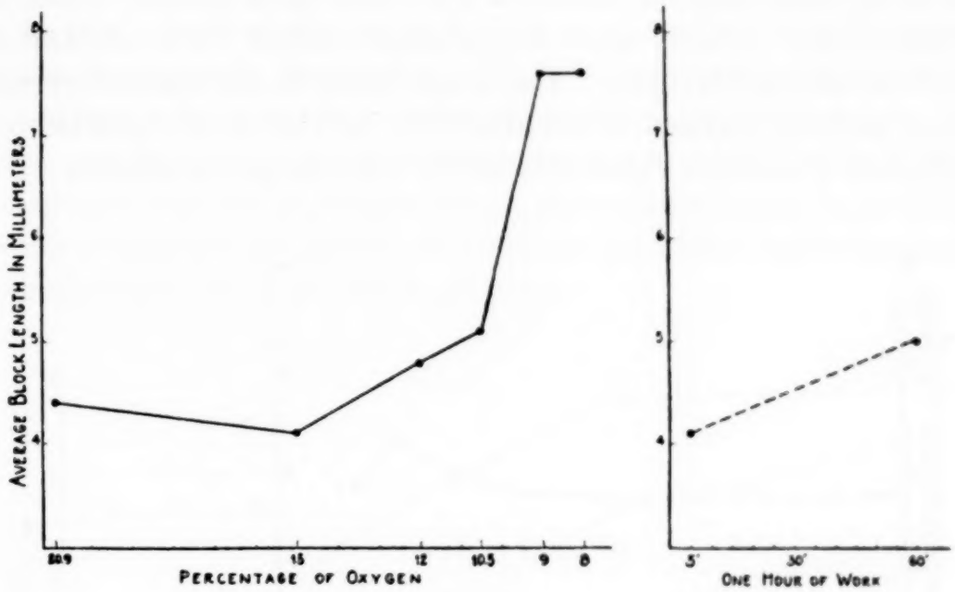


FIG. 2.

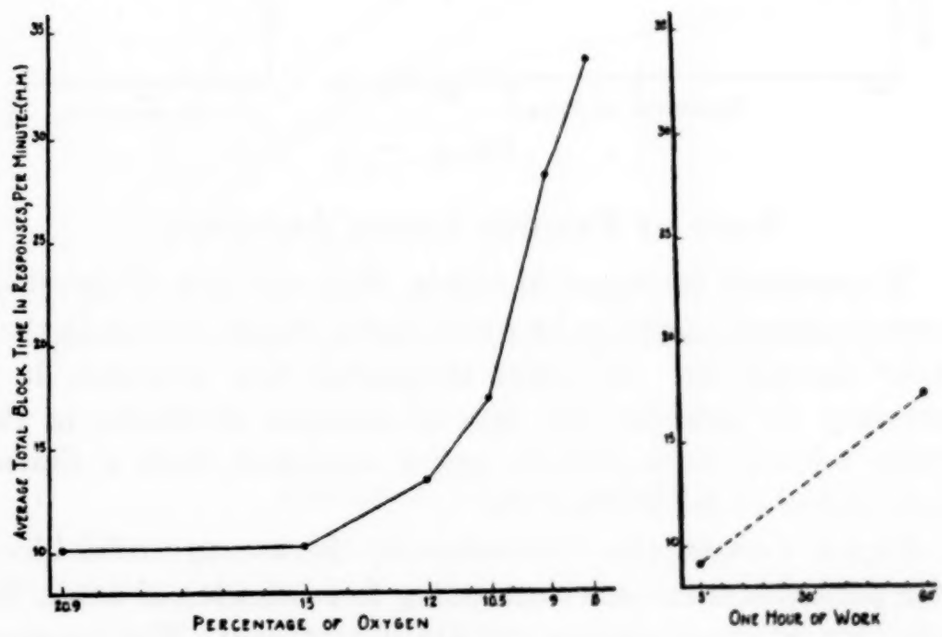


FIG. 3.

oxygen percentages. In fact the rise does not begin until the oxygen has been reduced below 15 percent. This finding is consistent with previous work on the behavioral effects of

anoxemia, for example McFarland's (**8**), in which the critical change occurred in the neighborhood of 13 percent oxygen.

The curves in Fig. 4 bring out graphically the fact that has already been stated, namely: that the basic modal reaction time of the subject does not change, either from anoxemia or from mental fatigue. The irregularity in the curve beyond 10.5 percent oxygen is attributable to the small number of subjects who took these extremely low oxygen mixtures.

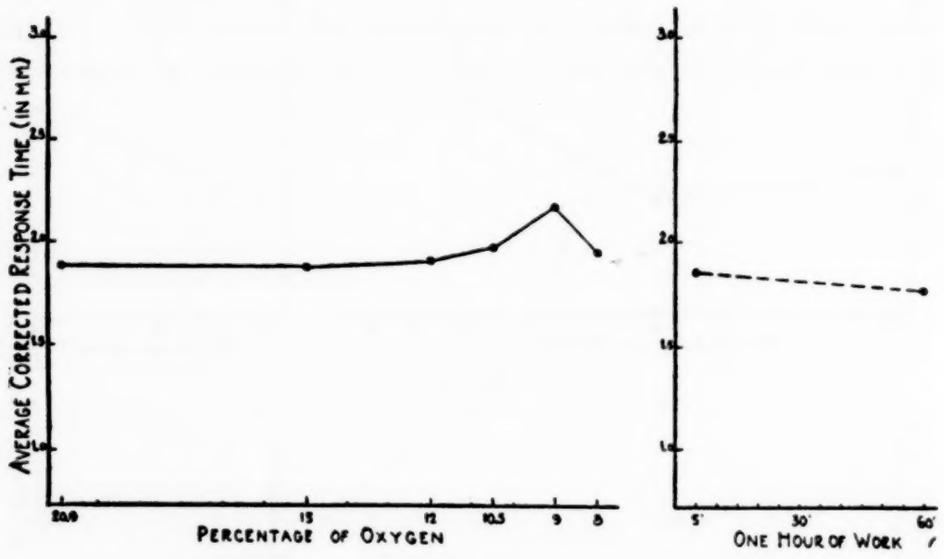


Fig. 4.

Rate of Fatigue under Anoxemia

If anoxemia increases blocking, then the rate of development of fatigue ought to be much faster under anoxemia than under normal air. In order to answer this question, it is necessary to compare the rate of increase of blocks in the fifteen minute work periods under anoxemia with a similar work period under normal air.

Figure 5 shows (for four subjects) the average total block time per minute in each succeeding five minutes of work, for each percentage of oxygen and for normal air. The assumption is made that the blocking starts at zero; hence, dotted lines are drawn to indicate the hypothetical rate of increase immediately after work begins. It is evident that the rapidity of the increase in blocking is inversely proportional

to the oxygen percentage breathed by the subjects, thereby corroborating the theory. This rule does not hold for the lowest percentages of oxygen, however, because the subject attains such a high degree of blocking immediately after work is begun under these conditions that the curve tends to become asymptotic thereafter. This is even more true of the 8 percent and 9 percent conditions which are not shown on this graph. A possible contributing factor to this is the adaptation to the anoxemia which the subject seems to acquire while continuously subjected to it, so that he is most severely handicapped by it at the beginning.

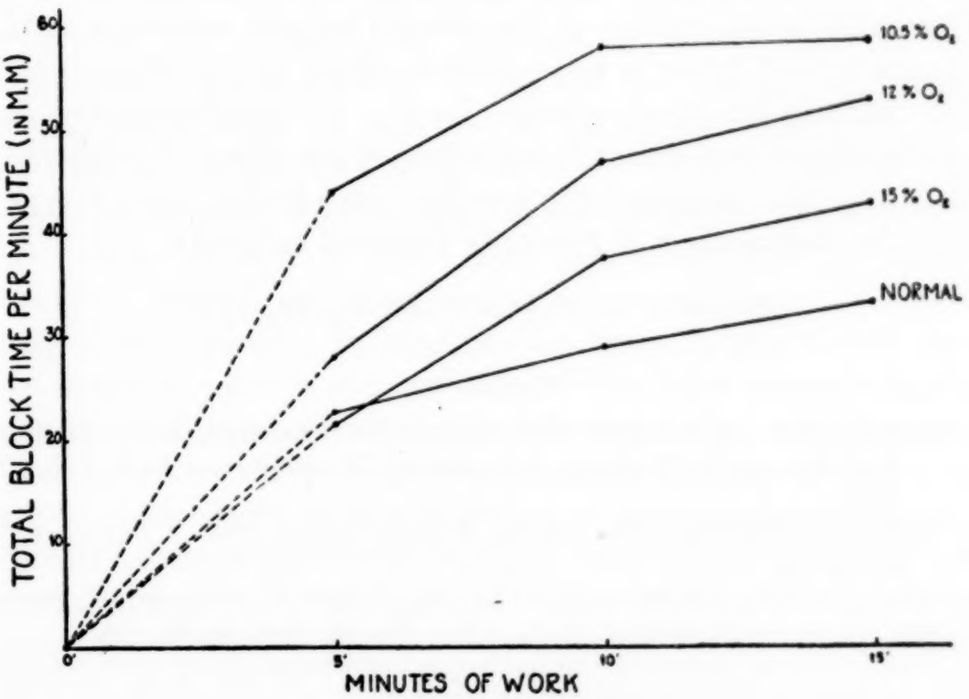


FIG. 5.

GENERAL CONCLUSIONS

1. When the oxygen content of the inspired air is reduced by increasing the ratio of nitrogen to O_2, there is produced an increase in the length and frequency of blocks in mental work (color naming) which is proportional to the amount of reduction of oxygen.

2. An hour's continuous mental work (color naming) in normal air produces an increase in the length and frequency

of blocks which is comparable to that produced by reducing the oxygen content of the air to 10.5 percent.

3. No marked change is produced in the modal response time of the subjects either by anoxemia or an hour of continuous mental work.

4. The rate of development of fatigue, as indicated by the rate of increase in blocking, is much greater under the anoxemia conditions than under normal air.

The above conclusions give some corroborative evidence for the theory that the physiological basis of mental fatigue is, in part at least, a reduction of the available oxygen supply to the functioning mechanisms. The assumption would be, then, that a restoration of the normal oxygen available to the tissues would cause a recuperative effect in the direction of the unfatigued performance level. A supplementary investigation is at present being made of the effect of artificially restoring the depleted oxygen by introducing pure oxygen into the inspired air of mentally fatigued subjects.

(Manuscript received February 18, 1937)

References

1. Benedict, F. G. and Benedict, C. G., *Mental Effort in Relation to Gaseous Exchange, Heart Rate, and the Mechanics of Respiration*, Carnegie Inst. of Wash., Pub. 446, 1933.
2. Bills, A. G., Blocking: A new principle of mental fatigue, *Amer. J. Psychol.*, 1931, **43**, 230–45.
3. Bills, A. G., Causal factors in mental blocking, *J. Exper. Psychol.*, 1935, **18**, 170–85.
4. Bills, A. G., A new psychergometer, *J. Gen. Psychol.*, 1936, **14**, 487–489.
5. Bills, A. G. and Shapin, M., Mental fatigue under automatically controlled rates of Work, *J. Gen. Psychol.*, 1936, **15**, 335–347.
6. Gerard, R. W., Studies on nerve metabolism, *Amer. J. Physiol.*, 1927, **62**, 381.
7. Lennox, W. G., The cerebral circulation. XV. The effect of mental work, *Arch. Neurol. and Psychiat.*, 1931, **26**, 725.
8. McFarland, R., The psychological effects of oxygen deprivation on human behavior, *Arch. of Psychol.*, 1932, **145**.
9. Rounds, G. H., Schubert, H. J. P. and Poffenberger, A. T., Effects of practice on the metabolic cost of mental work, *J. Gen. Psychol.*, 1932, **7**, 65.

ADAPTATION, AFTER-EFFECT AND CONTRAST IN THE PERCEPTION OF TILTED LINES. I. QUANTITATIVE STUDIES

BY JAMES J. GIBSON AND MINNIE RADNER
Smith College

The Problem

An essential element in visual perception is one indicated by the terms edge, boundary, contour or line. Things are seen because they are delimited from the rest of the visual field and this delimitation depends on, or consists of, the formation of a visual *line*. A line has the function of enclosing, in addition to its function of simply delimiting, but we are here concerned with it in the latter function only. Conceived in this way a visual line, or any designated portion of a line, may be said to have two characteristics: its *shape*, straight, curved, or 'bent,' and its *direction*, vertical, horizontal, or oblique. It has already been demonstrated that the shape of a line is subject to what has been termed adaptation and negative after-effect with respect to rectilinearity. That is, a curved or bent line-segment changes during continuous perception in the direction of becoming straight, and thereafter an objectively straight line appears curved the opposite way.[1] By analogy, the direction of a line might be expected to behave in the same manner with respect to the vertical and the horizontal axes. Experimental test of this possibility bears out the expectation; that is, a line seen as tilted somewhat from the vertical or the horizontal axis appears progressively less tilted during the course of perception, and a line objectively vertical or horizontal appears thereafter as tilted in the opposite direction.[2] In short,

[1] J. J. Gibson, Adaptation, after-effect and contrast in the perception of curved lines, J. Exper. Psychol., 1933, 16, 1–31. Also J. F. Bales and G. L. Follansbee, The after-effect of the perception of curved lines, J. Exper. Psychol., 1935, 18, 499–503.

[2] J. J. Gibson, *op. cit.*, 29, footnote 23, and Vertical and horizontal orientation in visual perception (Abstract), *Psychol. Bull.*, 1934, 31, 739. Also M. D. Vernon, The perception of inclined lines, *Brit. J. Psychol.*, 1934, 25, 186–196.

deviation of a line from the vertical or horizontal does produce an effect analogous to deviation of a line from the rectilinear.

If a subject seated squarely at a table is required to look for several minutes at a 20 cm line-segment drawn on a large square of cardboard, the line being tilted let us say 10° from the vertical, he will frequently but not always report spontaneously that the line seems slightly less tipped at the end of the period than it did at the beginning. If now a vertical line is substituted for the tilted one, he invariably reports that the line looks tipped the other way. The effect is slight but definite and may persist for several minutes. It occurs indifferently whether the subject fixates the mid-point of the line or simply inspects, *i.e.* looks at, the line during the period. Whether the line is tilted in a clockwise or a counter-clockwise direction makes no difference; the after-effect is always negative, *i.e.* in the opposite direction. If the inspected line deviates from the horizontal then an after-effect appears on a horizontal line; if from the vertical then on a vertical line.

Preliminary Experiments

In order to investigate this phenomenon, a series of experiments was undertaken. As a preliminary method of measuring the effect, a 20 cm line was drawn in india-ink on a large cardboard disk pivoted at its center to a rectangular upright screen. By rotating the disk the line could be set in any desired position. A scale, reading in degrees and visible from the rear through a slot in the screen, made it possible for E to record the position of the line. The amount of the negative after-effect could then be measured in the following manner. The line was set at the inclination desired for the inspection of the subject; at the end of the inspection-period E returned the line to the zero position, whereupon S corrected the apparent negative tilt by adjusting the line to that position at which it appeared to him vertical (or horizontal), that is at a degree of actual tilt just sufficient to counteract the negative after-effect. Hence the measured deviation from the objective vertical (or horizontal) was always in the same direction as had been the inspected tilt. A number of such measurements were taken with each subject, using in alternate periods lines tilted to the right and to the left in order to eliminate in the final average the influence of slight constant errors in the perceptual axis. The average of the measured deviations from zero obtained in such a series was taken as the amount of negative after-effect. Under these conditions variable errors were slight, and after-effects varying from 0.5 degrees to 3 degrees were found, depending on the subject and the amount by which the inspected line was tilted.

In our procedure the line was returned to the zero position before S made his adjustment. This was done in preference to having him adjust the line to vertical from the starting-point of the inspected tilt because of the tendency to stop short of the correct position when moving in toward it. This tendency, easily demonstrated by control tests, holds apparently in any experiment involving the reproduction of norms.

Several problems of an exploratory nature were investigated with the simple apparatus described. The fact was verified that, like the after-effect of curvature, the after-effect of tilt occurred following either *fixation* or *inspection* of the line. When the eyes were allowed to move freely along the line the effect was somewhat greater than when fixation was maintained at the mid-point[3] but such eye-movements were not necessary for the appearance of the effect. In the experiments which follow, inspection rather than fixation of the line was required of the subjects, chiefly for the reason that a confusing brightness-after-image was thereby avoided.

A preliminary search for the optimal degree of tilt showed that, although the negative after-effect appeared in measurable amount even when the inspected line was tipped only a few degrees from the reference-axis, it was more pronounced at greater inclinations. At deviations nearing 45°, however, little or no after-effect could be obtained on either a horizontal or a vertical line. On the basis of these observations the optimal degree of tilt for a maximum after-effect was estimated to be about 10° of inclination.

A number of tests were made with inspection-periods of varying duration which indicated that an appreciable after-effect could be obtained after a period as short as a minute. The occurrence of the phenomenon did not seem to depend on the distance between the eyes and the line, nor on the length of the line itself. If, on the other hand, a short tilted line were used for inspection and a long vertical line for the test, or *vice versa*, the after-effect was much reduced or even non-existent; the subjects' reports were variable rather than consistent.

It was found that a margin or edge, such as that between a black and a white area, would serve as well to show the phenomenon as would a black line on a white ground. Finally, if a tilted rectangular cross made of two lines intersecting at their mid-points were substituted for a single line, then the negative tilt would appear on *both lines* of a similar but normally oriented cross. In other words, the after-effect could be made to appear on a horizontal and a vertical line at the same time—a fact which raised the problem of whether the horizontal and vertical axis are mutually dependent or are to some extent independent.

Most of the phenomena which have been described may be verified without difficulty with no more elaborate materials than a pencil, ruler and some sheets of heavy paper or cardboard. The tilted line or lines and the subsequently inspected upright counterpart may be drawn in the central areas of two separate rectangles. The former should be inspected for two minutes or more and then the latter quickly substituted. The after-effect on a straight line of curvature or 'bending' may also be observed with this simple procedure.

In the experiments so far described, the tilted adaptation-line was always seen against a rectangular background. The negative after-effect likewise appeared on a line seen within this rectangle and framed by it but the effect did not, so far as could be noted, appear on the horizontal and vertical edges of the ground itself. So long, at least, as the objectively vertical line was looked at, it appeared tilted and the background upright; if an edge were looked at directly there might appear a slight effect but the subjects' reports were hesitant and doubtful. As compared with the after-effect of curvature, the tilt phenomenon could not so readily and certainly be observed

[3] This fact was also noted with the curvature-phenomenon (Gibson, *op. cit.*, 11f.). Bales and Follansbee found, in addition, that the curvature after-effect *persisted more strongly* if S read print for 60 sec. at the end of the inspection than if he merely fixated a point (*op. cit.*, 502).

on lines of a different length formed by the edges of doors and windows or by the corners of the room. In other words the phenomenon had so far been observed only in a visual field the periphery of which included objectively vertical and horizontal lines which remained relatively stable.

The experiment was now varied by requiring the subject to look through a tube during the period of adaptation and test—a procedure which eliminated these peripheral visual cues for vertical-horizontal orientation. The occurrence of the after-effect was not affected by this change. An objectively vertical line looked as definitely tilted as it had before, and it could be adjusted to a phenomenally vertical position with as much certainty as before. Neither the phenomenon of tilt-adaptation nor the more basic fact of vertical-horizontal orientation of a line in the frontal plane would seem to be necessarily conditioned by the presence of visible reference-lines in the field. Evidently we carry around with us our own visual reference-axes with respect to which a line may be seen as upright or tilted; in short we possess a 'sense' of visual direction.[4] Judgment of visual uprightness or tilt under certain circumstances is absolute; a line perceived in an otherwise homogeneous field has *intrinsic* direction.

In subsequent experiments the restricted circular field demanding absolute judgment of direction was used, rather than the unrestricted field which included upright lines making relative judgment possible.

The preliminary tests described had served to verify the general phenomenon and indicate a few of its main features. But chiefly they posed questions which required formal and quantitative investigation. Of these questions we undertook to answer the following, devoting an experiment to each.

First, what is the average acuity in detecting deviations of a line from the vertical or horizontal; with what accuracy can these fundamental directions be produced in a homogeneous circular field of view? Furthermore what is the difference, introspectively and quantitatively, between the production of horizontal or vertical lines, and lines at specified degrees of inclination—more especially the 45° position?

Second, what is the course of the adaptation-process in time? Does the degree of adaptation toward the vertical or horizontal increase with longer and longer periods of inspection and does complete adaptation ever occur, or not? So far, no subject had reported that a tilted line appeared completely vertical even after looking at it for a considerable period. In order to answer these questions, the amount of after-effect had to be measured after varying durations of inspection.

Third, does a line tipped from the vertical produce a negative after-effect on the horizontal in addition to the known effect on the vertical itself? If so, will the effect be in the same direction as on the vertical? Are the two axes, in short, modified as a unit; are horizontal and vertical mutually determined within a single system of spatial orientation or are they relatively independent?

Fourth, what is the relationship between the amount of negative after-effect and the degree of tilt, starting with 0° and increasing the inclination up to 90°? Where is the optimum, and why does 45° yield no after-effect?

These four problems will be dealt with in the present report. Two additional problems are postponed for subsequent treatment. The first is the question of whether

[4] Helmholtz for example reports an experiment in which a line, seen with one eye through a tube, could be set to the vertical position with some accuracy. This accomplishment was independent of the position of the eyes in the head; that is, it was undisturbed by a slight objective tilt of the retinal meridians (*Physiological optics* (Tr by J. P. C. Southall), Vol. 3, 256).

simultaneous contrast between neighboring regions of the visual field can be shown to operate in the perception of tilt. The second is the problem of whether the negative after-effect is akin to an after-image in being confined to the stimulated area of the visual field or is a generalized shift affecting the vertical and horizontal norms uniformly throughout the field. Observations already described pointed to the former alternative. If this were established, it would be difficult to maintain that the after-effect was an 'illusion of judgment' or a cognitive process of any sort.

The Major Experiments

Apparatus.—In order to measure the after-effect more accurately than had hitherto been possible, an apparatus was constructed similar in principle to the cardboard device already described. It consisted essentially of a large upright disk mounted at its center so that it could be rotated. The front bore a black line on a white ground; the rear a scale in degrees. The subject's vision was so restricted that he could see the black line bisecting a circular field (concentric with the disk) having a visual angle of 75°. He could grasp the edges of the disk with both hands, outside the field of view, and rotate the line about its mid-point to any required position, which it would maintain.

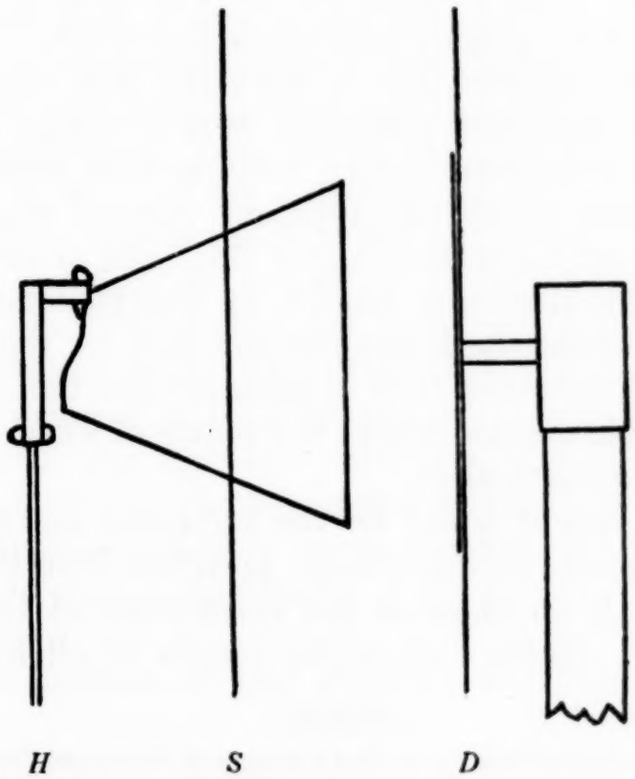


FIG. 1. The apparatus (*H*, headrest. *S*, screen with cone. *D*, adjustable disk).

The disk was made of reinforced beaverboard, 1 meter in diameter, and was mounted at its center to a heavy support. Attached to the front was a circle 56 cm in diameter of dull white drawing paper bisected by a line ½ cm wide. In front of the disk at a fixed distance the subject sat with his forehead and chin supported by a headrest, and the upper part of his face fitted into the small end of a cone of white cloth-paper permitting binocular vision. The axis of this cone would fall on a horizontal line drawn from the mid-point of the line to a point between the subject's eyes, a dis-

tance of 36 cm. The cone passed through a screen which served to cut off vision of the apparatus between trials and also to make rigid the cone itself. The subject's field, which was illuminated dimly and evenly by reflected light, included the circular rim of the cone and within it the circular disk of white paper bearing the line. The scale on the rear of the apparatus was of such size that readings could be taken to the nearest tenth of a degree. It was calibrated to the objective position of the line with a precision greater than this amount. A diagram of the apparatus is given in Fig. 1.

I. The Relative Accuracy with which Frontal Plane Directions may be Produced

Tests were made of the accuracy with which subjects could set the line at a horizontal and at a vertical position, and, for comparison, at 45°, 30° and 60° from the vertical. The adjustments were always made from a starting position 5° from the required position, alternately from the right and from the left. Five subjects who were to serve in subsequent experiments were given 8 tests at each position.

The results showed that in the visual field employed, the vertical and horizontal directions were produced with slight but consistent constant errors, varying from subject to subject. In other words the apparent vertical might deviate from the objective vertical either clockwise or counterclockwise, but this deviation tended to remain constant. The constant error for the horizontal and that for the vertical of any one subject tended to be in the same direction. Constant errors varied in amount from a few tenths of a degree up to 2°, depending on the subject.

The measure of acuity for the horizontal and the vertical axes was taken to be the average deviation from the constant error, which is an index to the consistency of the subjects' adjustments. Table I gives the results of all five subjects

TABLE I

Average Deviations in Degrees of Five Subjects in Producing Visual Direction

Subj.	Vert.	Horiz.	45°	30°	60°
B	.32	.46	.95	1.86	.29
L	.44	.64	1.18	1.65	2.98
H	.17	.54	1.84	2.14	3.07
M	.26	.56	2.43	2.64	1.05
R	.23	.40	.77	1.65	1.30
Avg.	.28	.52	1.45	1.99	1.74

for each of the five positions. It can be seen that acuity for the vertical and horizontal is greater than for the other directions, whose variability is from 3 to 6 times as great. These figures themselves are based on too few data to merit complete reliance. Other investigators, however, find the same thing. Jastrow [5] has reported findings obtained under comparable conditions which are similar in all respects. Neal [6] has found that even in complete darkness, a luminous line can be set to vertical with an average variable error of less than one degree. J. Volkmann has determined (investigation to be published) that under the same circumstances of complete darkness, the ability to discriminate directions of a line is very much higher in the immediate neighborhood of the horizontal and vertical than in other positions.

This fact of their greater perceptual accuracy would itself argue for the special character of horizontal and vertical lines, but the subject's introspective reports in our experiment are equally significant. They stated that the method by which a 30°, 45°, or 60° line was produced was to imagine a vertical and horizontal line in the field and then estimate in terms of one-third, one-half, or two-thirds of the imagined angular area. In short the vertical and horizontal directions *had to be present implicitly before other directions could be produced.* It may be concluded on both factual and *a priori* grounds that the up-down and right-left directions are fundamental, or better are *norms* for the perception of direction in the frontal plane.[7] Finally, it may be noted, these *normal directions* may be produced with a high degree of accuracy.

II. The Course of the Process of Tilt-Adaptation in Time

The amount of negative after-effect as a function of the time of inspection was next investigated. The five subjects

[5] J. Jastrow, On the judgment of angles and positions of lines, *Amer. J. Psychol.*, 1893, **5**, 220ff. The present data also corroborate Jastrow's finding that the 45° position is produced with a large constant error toward the horizontal.

[6] E. Neal, Visual localization of the vertical, *Amer. J. Psychol.*, 1926, **37**, 287–291.

[7] *Cf.* M. Radner and J. J. Gibson, Orientation in visual perception; the perception of tip-character in forms, *Psychol. Monog.*, 1935, **46**, No. 210, 63.

of the previous experiment were each given six tests at every one of the following durations: 1, 5, 10, 20, 45, 90, and 120 sec. A line tilted 5° from the vertical was inspected in one series of tests and the experiment was then repeated with a line tilted 5° from the horizontal. Three of the five subjects were ignorant of the phenomenon being investigated; at all times the subjects were not permitted to know their results. In all, 420 tests were run off. A test consisted of (*a*) the inspection of the line, (*b*) a one second interval during which S closed his eyes and E set the line at approximately zero on the scale, and (*c*) the adjustment of the line by S within 8 sec. to his phenomenal vertical (or horizontal). The deviation of the scale-reading from zero, together with its direction, was recorded.

Rest-intervals were introduced between tests varying in length from 15 sec. after the shortest inspection-periods to 3 min. after the longest. In order to avoid any possibility of a cumulative negative after-effect, and also in order to enable the data to be treated without correcting for a constant error, tests were given with the inspection-line tilted alternately to the right and to the left of the reference-axis. Of the 6 tests at each time of inspection, 3 were inclinations in one direction and 3 in the other. These two sets of scale-readings were averaged separately and the negative after-effect was computed as half the angular distance in degrees between the two averages. Normally this meant merely averaging the two, since they were on opposite sides of the absolute vertical, but occasionally a subject's constant error in one direction would be greater than his average after-effect, in which case it meant averaging the difference between the two. It was discovered that a subject's constant error shifted somewhat from day to day. The procedure of alternating tests to the left and right of the vertical with only short rest intervals probably reduced the obtained amounts of negative after-effect, but this was safer than a procedure which might have enhanced them.

Results.—The data of the experiment, averaging all 5 subjects together are shown graphically in Fig. 2. The individual graphs differ from the average only in showing

less regularity. After one second of inspection of the tilted line there was no after-effect (.09° for the horizontal and − .008° for the vertical). After the 5 sec. period, however, an appreciable effect appears of .28° for the vertical and .38° for the horizontal. The curve of the effect rises rapidly with increasingly larger periods until 45 sec., beyond which it continues to rise but very slowly. It is notable that adaptation occurs fairly quickly; a 20 sec. inspection suffices to produce a shift of nearly 1° which is a quite noticeable tilt under normal circumstances. The process of color-adaptation which produces ordinary negative after-images is not much faster than this.

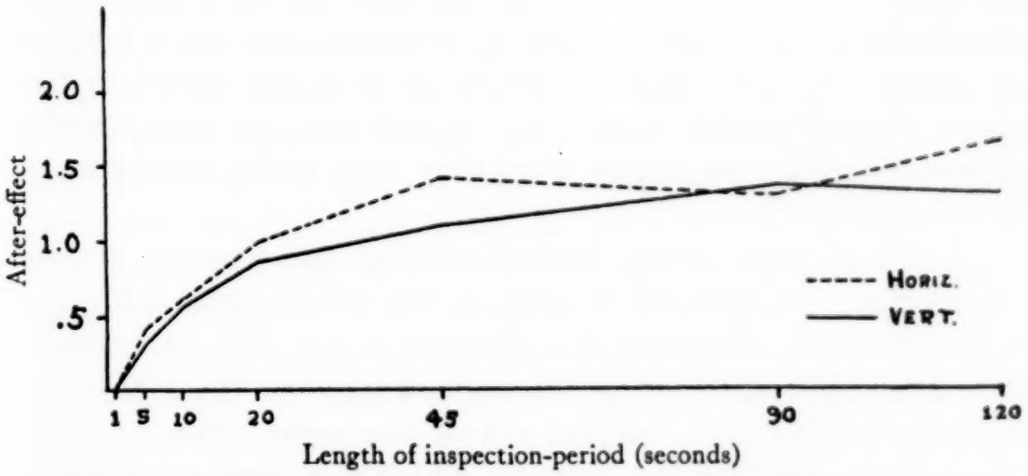


FIG. 2. Negative after-effect on a vertical and a horizontal line as a function of increasing times of adaptation.

A second notable feature of the curve is its asymptotic shape. The process of adaptation apparently tends to level off at an average of about 1.5° for the subjects tested. The individual curves, although less regular than the average curve, show this same characteristic. Several tests with 5 and 10 minute inspection-periods yielded after-effects not much higher than the average obtained at 120 sec. As a crucial test, one additional subject inspected a line tilted 5° to the right of the vertical for 30 minutes, repeating the experiment the next day with a line tilted to the left. The average of these two results was almost exactly the same as that obtained by this subject with a 90 second inspection

(approximately 1°). It may be concluded that under our conditions a perceived tilt does not reach complete adaptation, *i.e.* disappear entirely, but rather that after a few minutes of inspection a virtual equilibrium is attained in which either the perceived line has shifted toward the reference-axis or the reference-axis has shifted toward the perceived line. Curvature-adaptation also behaves in this way, and it is interesting to note that localized color-adaptation ordinarily does not reach zero saturation, although Hering believed that it should do so.[8]

The possibility advanced at the beginning that a cumulative effect might appear with a series of tests in the same direction is rendered unlikely in the light of the two 30 min. inspections. As a further check, however, one subject made 12 successive one minute inspections of a line tilted to the right, at half-minute intervals. At the end, his after-effects were averaging very nearly what they were at the beginning—in this case 1.7°.

A comparison of the horizontal and the vertical curves shows that the after-effect was on the whole greater for the horizontal axis, although the differences may not be reliable. The average deviations in this experiment for the 6 tests at each period for each subject are of the same order as were those of the previous experiment, which indicates comparable accuracy. At the 45 sec. period for example they averaged 0.45° and 0.29° for the vertical and horizontal respectively.

It would be interesting if the curve of the disappearance of the after-effect in time could be compared with the foregoing curves of its development. Unfortunately no systematic experiment has been done on the persistence of the effect. It may be said with certainty that the after-effect does not disappear immediately after inspection has ceased, and it is the impression of the writer that the longer the inspection-period has been, the more strongly does the after-effect persist. Vernon [9] found that after a 10 minute inspection, the effect on the vertical was still appreciable 5 minutes later.

[8] L. T. Troland, The colors produced by equilibrium photopic adaptation, J. Exper. Psychol., 1921, 4, 344–390.

[9] M. D. Vernon, The perception of inclined lines, *Brit. J. Psychol.*, 1934, 25, 189.

III. The Interdependence of the Horizontal and Vertical Directions

The third experiment was undertaken in order to determine whether a negative after-effect obtained on one reference-axis transferred to the other; whether horizontality and verticality function as norms relatively independent of one another, or whether they function together as merely two aspects of a single system for visual orientation. In the latter event a shift of one reference-axis, clockwise or counter-clockwise, should be accompanied by a similar shift of the other axis.

From the data of the previous experiment the average after-effect on the vertical axis of a line tilted 5° from the vertical was manifested and also the effect on the horizontal axis of a line tilted from the horizontal. We may call these *direct* after-effects. Using these results at the 45 sec. period of inspection we now had to obtain for comparison results with the same 5 subjects for the after-effect on the *horizontal* of a line tilted 5° from the *vertical* and the effect on the *vertical* of a line tilted from the *horizontal.* These are *indirect* after-effects. No change in the procedure of the experiment was made except to test the other axis of reference. Six tests were given each subject, as before, alternating the direction of tilt of the inspected line.

Results.—Averages of the 6 scale-readings were computed as before and are given for each subject individually in the second and fourth columns of Table II. The figures in the

TABLE II

A Comparison of Direct and Indirect After-Effects in Degrees Following a 45-Second Inspection of a 5° Tilt

Subj.	5° Tilt from vertical		5° Tilt from horizontal	
	Direct effect on vert.	Indirect effect on horiz.	Direct effect on horiz.	Indirect effect on vert.
S	1.47	.49	2.55	.62
L	.98	1.03	1.30	.85
H	.68	.03	.97	.74
M	1.12	.45	.70	.55
R	1.22	.70	1.60	.97
Averages	1.09	.54	1.42	.75

first and third columns are taken from the last experiment. The indirect after-effects are in every case deviations *in the same direction* as the direct after-effects. In other words the figures show that for all subjects when the phenomenal vertical had become tilted in a certain direction from the objective position, the phenomenal horizontal had also become tilted in the same direction and *vice versa*. These transferred after-effects are in general, however, smaller than the direct effects. In this respect the individual averages of the table show some regularity, with the exception of the results of subject L in the second column which indicates an indirect after-effect even larger than the direct effect. Subject H yielded almost no indirect effect whatever. But the remaining results are consistent with the group averages at the bottom of the table. They point to the conclusion that an after-effect on one axis is accompanied by a corresponding indirect after-effect on the other axes, and that the indirect after-effect is not as great as the direct after-effect.

The answer to the question put at the beginning of the experiment would seem to be that perceptually the horizontal and vertical directions do behave as if they were aspects of a single system for visual orientation, or a single *spatial framework* [10] but that on the other hand they are not rigidly linked together; there is, as it were, a certain amount of 'play' or 'lost motion' between them. Presumably therefore they may function as spatial standards with a certain limited degree of independence.

IV. The Negative After-Effect as a Function of the Degree of Tilt of the Inspected Line

A line tilted clockwise from the vertical produces an after-effect on both the vertical and horizontal axes such that they must be objectively tilted clockwise in order to appear normal. But if the line to be inspected is inclined still more in a clockwise direction it may reach a point at which it deviates perceptually not clockwise from the vertical but counterclockwise from the horizontal. In this case, as

[10] K. Koffka, *Principles of gestalt psychology*, Harcourt Brace, 1935, 211ff.

we have seen, the after-effect on the horizontal and vertical is in the opposite direction to what it has been before. In other words, a line may change from one kind of perceived tilt to another as it is rotated from 0° through 90° and there is a correlative change in the negative after-effect. This principle is, in fact, only to be expected if tilt is a deviation from normal orientation *in a certain direction*, and if a line may be normally oriented when either horizontal or vertical.[11] At 45°, half way between the two, the perceived tilt should be equivocal; at this point it is perceptually changing its reference from one norm to the other. In a psychological sense there is a limit to the amount of tilt which a line may bear.

In the following experiment the negative after-effect on the vertical axis was measured for inclinations up to 90°. Beyond 45° an indirect after-effect was to be expected in accordance with the outcome of the last experiment. The adaptation-line was inspected at the following inclinations: 2.5°, 5°, 10°, 20°, 30°, 45°, 60°, 70°, 80°, 85°, 87.5°. Six tests were made at each inclination, as before, alternately to the right and to the left of the vertical. The length of each period of inspection was 1½ min., with a rest interval of 2 min. Two subjects were employed in this experiment who had not taken part in the previous experiments. They were tested alternately one during the rest-period of the other. Subject J began with the 2.5° line and proceeded to greater degrees of tilt, while G was given the reverse procedure. This precaution against any progressive habituation proved to be unnecessary, however.

Results.—Computations of the after-effects were made in the same manner as before. The averages for both subjects separately were plotted and are shown in Fig. 3. At a tilt of only 2.5° from the vertical an appreciable after-effect appears. At 5° for subject G and at 10° and 20° for subject J, a maximum is reached, beyond which the after-effect declines

[11] Visual forms possessing a contour, and other visual patterns less elementary than simple lines, may pass from one normal orientation to another with a smaller rotation than one of 90°. A square, for example, becomes a normally oriented diamond at 45°. *Cf.* M. Radner and J. J. Gibson, Orientation in visual perception; the perception of tip-character in forms, *Psychol. Monog.*, 1935, **46**, No. 210, 49f.

with increasing degrees of tilt, until at 45° the effect has disappeared for one subject and is almost negligible for the other. Beyond 45°, an after-effect in the opposite direction begins to show itself, since the line now begins to be tilted not to the right (or left) of the vertical but to the left (or right) of the horizontal. The curve of this indirect after-effect is roughly symmetrical with the preceding curve of the direct effect; it has the same relationship to the horizontal axis that the direct curve has to the vertical axis. If the two axes are

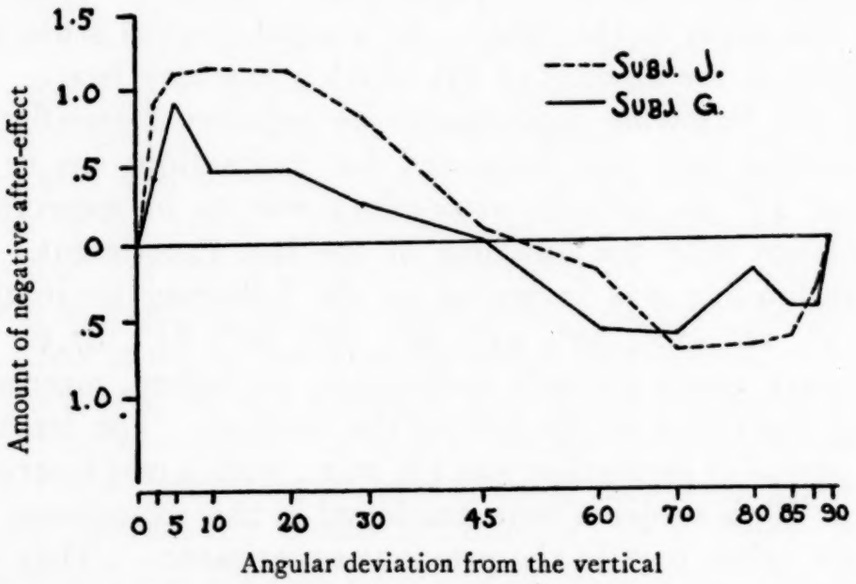


Fig. 3. Negative after-effect in degrees on the vertical axis as a function of the objective deviation from that axis of the inspected line (points above the abscissa indicate a shift of the subjective vertical toward the inspected line; points below indicate a shift in the opposite direction).

in part interdependent, this is only to be expected. The chief difference between the curves lies in the fact that the 45° to 90° effects are considerably smaller than the 0° to 45° effects. This fact also tends to corroborate the finding of the preceding experiment in which indirect effects were smaller than direct effects.

Summary

It has been shown that not only the shape of a visual line but also its direction undergoes adaptation with negative after-effect. These qualities of curvature and of tilt may be

considered fundamental; they are the characteristics with respect to which a line may vary and by specifying these characteristics one defines the line either as a whole or in any given portion.

The norms of vertical and horizontal are discriminated and reproduced with considerable accuracy. Variable errors did not rise above .6 of a degree. A tilt of less than one degree, therefore, was perceptible to all subjects.

By measuring the amount of linear tilt-adaptation it was shown that it increases with continued inspection of the line in a time-curve similar to those of other processes of adaptation. Tilt-adaptation is not complete but instead levels off long before the quality of tilt is eliminated.

A negative after-effect on one reference-axis is accompanied by a corresponding indirect effect on the other axis, less in amount than the direct effect.

A line may be tilted up to 45° in either direction from the horizontal or vertical norm. The tilt may vary within fairly definite limits and it may be described in terms of two opposite qualities with a normal quality between them.

(Manuscript received September 24, 1936)

REACTION TIME AND CONDITIONING: EXTINCTION, RECOVERY, AND DISINHIBITION

BY CARL N. REXROAD

Stephens College

In the first report of these studies,[1] conditioning and its associated phenomena seemed to be clearly indicated. The procedure used was the presentation of a secondary stimulus (buzzer) followed in three seconds by a stimulus (flash of light) to which the subject had been instructed to react by making a full revolution of a crank, with reaction time measured and taken as an index of readiness to respond to the light. For convenience, these training presentations are designated *S–P*, while test presentations consist of some appropriate variation from the regular presentation.

In the first studies, automatic recovery was not investigated, and further data with reference to experimental extinction and to disinhibition were obviously needed. This paper is primarily concerned with these phenomena, but since modified apparatus has been employed, it is necessary first to note the changes and the effects of these changes on results.

Apparatus

A synchronous motor drives a rotor at the rate of one revolution each twelve seconds over a set of contacts in a stator which can be manually rotated. Contacts are available on the stator for each half second of a four second space, the remaining space of the stator being blank. Each contact lasts ¼ second, and by means of a set of switches any one or any combination of four stimuli—a buzzer (*S*), a light (*P*), a bell (*E*, extra), or a tickler (*T*)—can be activated through any one of the contacts. The subject is situated in a partially soundproof room, and on the table before him is the crank which he is to turn, the light to which he is to react as quickly as possible, and a covered buzzer and bell. He holds a solenoid magnet (tickler) in his left hand, keeping the thumb and finger over the ends of the tube. (This tickler has been added to the apparatus since the first studies.) As the subject turns the crank, a contact is

[1] Reaction time and conditioning: first studies, J. Exper. Psychol., 1936, **19**, 144–158.

broken and the counter is stopped. Also the turning of the crank moves a rotor over ten contacts and flashes a light before the experimenter, so that 'false' turns can be noted.

The control apparatus gives the standard presentations automatically, except that the experimenter irregularly rotates the movable stator while the rotor is on dead space. In this way the interval between presentations can be varied from 2 to 16 seconds. Without movement of the stator the interval is 9 seconds, and with movement the intervals will average 9 seconds. The introduction of this movable stator overcame a serious defect of the apparatus first used. With it the interval between presentations was constant, so that it might be held that subjects were merely learning to be ready for the primary stimulus every ten seconds (the time required for a revolution of the rotor on the old apparatus) rather than learning to expect the light three seconds after the buzzer. The introduction of the irregular interval has had no observable effects on results, and in consequence, it can be held that subjects in the first study as well as in later studies were conditioned to the buzzer, not the interval between presentations.

A second defect of the first apparatus was that the duration of the *P* stimulus or any stimulus substituted for it was dependent upon the subject's reaction time. With the new apparatus the duration is always ¼ second. More serious, however, was the fact that if with the old apparatus the subject moved the crank from its rest, the primary stimulus would fail to appear, *i.e.*, an extinctive presentation would be introduced at undesired times. If now the sujbect moves the crank from its rest before starting his response, reaction time is not recorded for that presentation, but as far as the subject is concerned, the presentation and response are identical with any presentation and response.

Procedure

After a subject was instructed as to his task, he was given fifteen standard presentations and then was shown his reaction times on these trials. He was next given 17 or 18 presentations including one test presentation and again shown his reaction times. If he expressed concern about his slowness after the test presentation, he was told that these reaction times would not be counted in calculating his average, that these irregular presentations were a part of the experiment and that he should do the best he could when they were introduced. He was then given four more sets of 17 or 18 presentations with one test in each set and with a brief rest between sets. Thus each subject received five test presentations, and these various tests were introduced for some subjects in one set and for others in another set, so that the results for a test of one type would be as nearly comparable as possible with the results of a test of another type. One hundred and eighty subjects served, predominantly young women from the author's classes.

Results

A. Extinction

In the first studies it was shown that *S–O* (buzzer with light omitted), *S–S* (buzzer followed in three seconds by buzzer), and *S–E* presentations resulted in markedly slow reactions on the next few standard presentations. No attempt

was made to calculate the statistical reliability of this slowness, and due to the fact that different tests came at different stages of training, no direct comparison of the slowness resulting from each sort of test could be made. This comparison is highly important since only the *S–O* is a direct analogue of Pavlov's extinctive procedure. The making of this comparison was the prime purpose of the study reported here. Slowness after an *S–S* or an *S–E* might be due to the fact that the subject felt he had been tricked and would have to be on his guard against being tricked again into a false reaction, *i.e.*, to a conscious or verbally controlled reaction to the variation. Or it might be due to some unconscious, less controllable disruption of his set for reaction, a disruption comparable to that found by Pavlov and his associates. Slowness after an *S–O* would rather clearly be of the latter sort, unless possibly the presentation would cause the subject to become unduly concerned about a possible defect of the apparatus. Thus if the *S–S* results in a much greater slowing than an *S–O*, factors in addition to those involved in Pavlovian extinction are indicated, while if the *S–S* results in no more slowing than an *S–O*, one would seem justified in referring to this slowing as extinction.

Results show somewhat less slowing after *S–S*, *S–E*, and *S–T* presentations than after *S–O* presentations. The amount of slowing for each type of presentation is shown in the first four rows of Table I, while the fifth row shows the slowing for all four combined. The first row reads as follows: on the first *S–P* presentation after an *S–O* subjects were slowed 10.97/120ths of a second, a slowing which is 6.0 times as great as the sigma of the difference between the reaction times on these presentations and the regular *S–P* presentations, and a slowing which was shown by 88 percent of the subjects; on the second *S–P* after the *S–O* the slowing was 4.47/120ths of a second, etc.

As already noted, the disruptive influence of the *S–S*, *S–T*, and *S–E* is somewhat less than that of the *S–O*, but the differences in amounts are not statistically reliable, the reliability indexes for the various comparisons ranging from .4

to 1.3. Statistically, at least, the four presentations are comparable in extinctive influence, but if the greater extinctiv influence of the *S–O* were to be taken as a genuine difference, a plausible explanation offers itself. When the *S–O* is given, the subject gets tensely ready for the light which fails to appear, and he holds this readiness for a prolonged period. When an *S–S* is given, he gets ready for the light and when in its place the buzzer is sounded, he either turns and thus removes his tenseness, or he relaxes without turning, or he holds his tenseness for another three seconds to see if the light will

TABLE I

	Dif.	Dif. / s.dif.	Percent *S*	Dif.	Dif. / s.dif.	Percent *S*	No. *S*
1. (*S–O*) *S–P*	10.97	6.0	88	4.47	3.3	70	60
2. (*S–S*) *S–P*	10.60	4.9	87	3.73	3.4	70	60
3. (*S–T*) *S–P*	8.97	5.2	83	4.77	3.3	70	60
4. (*S–E*) *S–P*	7.62	4.7	72	4.13	3.3	70	60
5. (*S–V*) *S–P*	9.31	8.3	80	4.28	7.2	70	240
6. (*S–V*)——*S–P*	7.11	6.5	81	2.76	3.1	61	120
7. (*S–V*) *TS–P*	13.25		95	5.00		70	60
8. (*S–V*) *E*)*S–P*	11.71		87	6.41		72	60
9. (*S–V*) *SP*	40.80		100	6.85		85	20
10. (*S–S*) *ES*/*P*	22.00		100	6.30		90	10
11. *VS–P*	10.82		91	1.45		50	70
12. *V*)*S–P*	4.46		67	2.09		57	100
13. *SP*	35.60		100	3.80		75	20
14. *ES*/*P*	38.50		100	3.80		70	10

S, secondary (buzzer); *P*, primary (light); *E*, extra stimulus (bell); *T*, tickler (extra stimulus); *O*, omission; *V*, various, *i.e.*, *S*, *E*, *T*, or *O*; ( ), preceding presentation; –, three second interval between *S* and *P*;), one second interval, /, half second interval; ——, thirty-three second interval.

appear three seconds after the second buzzer. In any case, after an *S–S* he does not hold himself ready so long as after an *S–O*. From his observations and from reports made by subjects, the author is inclined to believe that the extinctive effect of an *S–O* is actually greater and that the explanation is in terms of the length of time during which the subject maintains a readiness for turning.

This explanation would seem to throw some light on the nature of the extinctive process. Extinction takes place

when the conditioned response is made and disintegrates instead of going on to completion. In Pavlov's experiments a conditioned readiness to eat is aroused but dies off without going on to actual eating, while in the present experiments, the conditioned readiness to turn is aroused but deteriorates without going on to actual turning. The secondary stimulus precedes this disintegration and so tends to become effective for causing disintegration rather than a readiness. This explanation would seem to fit into the fact that the extinctive effect decreases with training, as was shown in the first report. After subjects have been thoroughly trained and the light fails to appear after the regular three second interval, they immediately relax until the buzzer is again sounded. They know, if you please, that if the light fails to appear after three seconds, it won't come at all.

B. Automatic Recovery

In the first studies it was found that the disruptive influence of *S–O* and similar presentations lasted through the next two standard presentations. In the present studies tests for automatic recovery were given by allowing an amount of time equivalent to that involved in giving two succeeding presentations to elapse before giving further stimulation, as indicated in row 6 of Table I. Given this period for recovery, subjects still show a marked slowness on the first *S–P* and some slowness on the second *S–P*, but this slowness is not so marked as the slowness without the recovery interval, as can be seen by comparing rows 5 and 6. The difference between 7.11 and 9.31 is 2.1 times the sigma of the difference and between 4.28 and 2.76 is 3.1 times the sigma of the difference. In other words, there is unmistakable recovery but not complete recovery. It is to be remembered that these results are for subjects in the early stages of training. In fact, subjects given six sets of presentations in one day make only slightly greater progress than if given two sets. Also, because of imperfect balancing of presentations, the difference is probably somewhat greater than that shown. Early indications were that *S–O* and *S–S* presentations

would be equal in disruptive influence, and for the data of row 6, *S–O* presentations were used in half the cases, while in row 5, *S–O* presentations enter into only one-fourth the cases. Since the *S–O* presentations have a somewhat greater extinctive effect than the *S–S*, *S–E*, and *S–T*, it is probable that the values of row 6 would be less if the *S–O* had been used in only one-fourth instead of one-half the cases. But even with this imperfect balancing, recovery is unmistakably shown.

C. Disinhibition

The data of rows 7–10 were collected on the assumption that if disinhibition could be shown in these studies, it would be shown by at least one of the four types of presentation indicated. By analogy with Pavlov's findings, the addition of an extra stimulus (bell or tickler) after an extinctive presentation should remove the disruptive influence of the extinctive presentation. But subjects are slower when the extra stimulus is added than when it is omitted, as can be seen by comparing rows 7–10 with rows 1–5. Rows 11–14 show the effect of introducing an extra stimulus not after an extinctive presentation as in rows 7–10 but somewhere in the regular series of *S–P* presentations. When the extra stimulus is introduced in the regular *S–P* series, it acts as an external inhibitor, and when introduced after an extinctive presentation, the effect seems to be to add external inhibition to extinction; the slowness of row 7 is greater than that of row 11, of row 8 greater than that of row 12, and of row 9 greater than that of row 13. An exception occurs for the presentation of *ES/P* but the author fails to see anything in these results to indicate disinhibition, while the results as a whole indicate that the extra stimulus acts as an inhibitor rather than a disinhibitor. If individual subjects are considered rather than averages for various subjects, then it may be reported that an occasional subject seems to show disinhibition on (*S–V*) *E*)*S–P* presentations. These subjects show a normal reaction time on the *P* of this presentation and a slowed time on the *P* of the next *S–P* presentation. So far only one such

subject has been checked to determine whether these reaction times are shown consistently, and this subject did show consistent normalcy when the extra stimulus was given, while without the extra stimulus she showed extinction. (She was not aware of the reason for the additional tests.) It may be that other like subjects can be found, but at present the evidence for disinhibition except for this one subject seems decidedly negative as was Skinner's evidence.[2]

Discussion

The data presented in this and the first report show phenomena at least analogous to conditioning, differentation, extinction, recovery, and external inhibition, and the phenomena are revealed through a study of a voluntary rather than a reflexive action. Stephens,[3] by a different technique seems to have shown conditioning of a voluntary response but not the associated phenomena. Gibson,[4] has raised the question as to whether Stephens' results show conditioning, and he is inclined to feel that the objections he raises with reference to Stephens' results might also be raised with reference to the studies here reported.[5] He holds that Stephens' conditioned responses are merely the familiar false reactions. In the studies here reported there are two sorts of reactions which might be regarded as false responses: (*a*) responses to the buzzer when it is sounded in a regular *S–P* presentation, and (*b*) response to any stimulus other than the light when this stimuluse is presented in place of the light, *i.e.*, on an *S–S*, *S–E*, or *S–T* presentation. False reactions of the (*a*) type have not been mentioned in these reports, but they are quite numerous on the first five or ten presentations and rapidly disappear as training progresses, being negligible in

[2] Skinner, B. P., A failure to obtain 'disinhibition,' *J. Gen. Psychol.*, 1936, 14, 127–135.

[3] Stephens, J. M., The conditioned reflex as an explanation of habit formation. III. The operation of two higher order reactions in close succession, J. Exper. Psychol., 1936, 19, 77–90.

[4] Gibson, J. J., A note on the conditioning of voluntary reactions, J. Exper. Psychol., 1936, 19, 397–399.

[5] Personal correspondence.

number after the 16th presentation. The author regards them as truly false reactions, and the regular training presentations are in a sense a training against such responses. Reactions of the (*b*) type are, however, interpreted as conditioned responses. They definitely increase in number with training, as shown in the first report, and as Gibson claims they should, if they are to be termed conditioned responses, and this in spite of the fact that throughout the training the subjects are learning not to respond to the buzzer as such. In these studies the buzzer becomes a signal for the light and induces a readiness to respond to the light. When this readiness is highly developed, any stimulus will trip it into overtness. The stimulus which trips it into overtness is not the conditioned stimulus, but rather the conditioned stimulus is the one which produces the readiness. Stimulating the subject when this readiness is at its height is merely a means of showing that the readiness is present. The standard presentations are such that the buzzer should, if subjects are conditionable, more and more fully set up this readiness, and the evidence is that it does, *i.e.*, the evidence is that subjects are conditionable.

Really all that is said above is that human subjects are capable of learning to use a buzzer, given regularly three seconds before a light, as a signal for the appearance of the light. The surprising thing to the experimenter is not that they learn this but rather that they are so slow in learning it. The buzzer is a distraction for approximately one-fourth the subjects even after the six sets of presentations given in this second set of studies. Their 'insight' is nil, and the amount of conditioning which they have undergone is below a critical value. Another fourth of the subjects realize that the light comes three seconds after the buzzer but are so afraid they will make a mistake and turn when the light is not present that they definitely keep themselves from getting highly ready for the light, from taking a motor set. Their 'insight' is partial, but they are slow and cautious and show little objective evidence of learning. There are, however, psycholo-

gists who object to using conditioning and learning as even approximately synonomous, and it has been the purpose of these studies to show that in learning to use a buzzer as a signal, subjects show the phenomenon associated with the conditioned response, to show, if you please, that learning and being conditioned are not so far apart. That in this learning subjects do show phenomena analogous to the conditioned reflex phenomena seems indisputable.

(Manuscript received August 12, 1936)

WHOLE VERSUS PART LEARNING THE SPIDER MAZE

BY THOMAS W. COOK

Acadia University

In order to investigate further certain aspects of the work of Cook, Morrison, and Stacey (1), and Cook (2), it seemed advisable to make a somewhat intensive study of the whole-part problem with 'blindfolded' maze learning. In the present experiment 38 patterns were learned by each of three subjects: W, a woman of 30; M, a male college student 20 years of age; and C, a girl 10 years old in the fifth grade. All were considerably above average in intelligence.[1]

Our approach to the whole-part problem implies that under favorable conditions intelligent understanding and immediate memory of a maze pattern may be dominant factors in elimination of errors. One such condition is that the unit to be mastered shall not exceed the subject's mental grasp. A second condition is that inability to remember the correct path be heavily penalized. The usual maze, however, presents too few alternatives (usually two) at each choice point to put a premium upon insight. It was accordingly decided to build a maze with 8 alternative paths at each choice point.

The main feature of this maze is a standard maze unit. The base of the unit is a block of softwood 4 inches square and 2 inches thick. The 4 inch square 'lower' surface rests on a table; on the centre of the upper surface are nailed two circular blocks cut from hardwood chair doweling, the larger 1 inch in diameter and ½ inch thick, the smaller 3/8 inch in diameter and 5/16 inch thick. These are mounted on the base pyramid fashion, the 1 inch block exactly in the centre of the 4 inch block, and the 3/8 inch block in the centre of the upper surface of the 1 inch block. Eight 7/8 inch staples are driven into the base, one end of each touching the 1 inch block at intervals of 45 degrees, and the other end extending towards the nearest corner or side of the 4 inch block. The whole looks like a caricature of a spider mounted on a block (the 4 inch base) with the staples representing the legs and the smaller blocks

[1] I hope the reader with a sense of the ridiculous will pardon the use of M, W, and C to signify man, woman, and child. Individual differences were of some importance, and there is an advantage in having labels that readily identify the subjects.

the body, hence the label: 'Spider Maze.' The remainder of the apparatus consists of connecting rods cut from ½ inch round chair doweling, and grooved ½ inch deep at each end to fit the projecting ends of the staples. Since the maze units are all alike, any number of comparable patterns can be constructed by arranging the maze units and connecting rods in various spatial patterns. A small screw slightly nearer the edge of the block than the projecting end of each staple could be turned 'in' or 'out' to bring the upper surface of connecting rod and staple to the same level. Each maze was assembled on a table 26 inches high, and was held firmly in position by small blocks inserted between the 4 inch blocks, and by glass push pins.[2] The subject was blindfolded by auto-goggles with cardboard eyepieces.

The forefinger alone was used for tracing; the rest of the hand was lightly closed. Preliminary verbal instructions explained the nature of the task, and stressed avoiding any thought of the maze pattern between trials. Written instructions seemed unnecessary, as all subjects traced several small practice mazes before beginning the experiment proper. Readings from a stop-clock gave time per trial, and from a similar clock the 20 second interval between trials. For the error record the staple nearest the subject (on any given block) was named 8, and the remainder numbered 1 to 7 clockwise. Retracing within a maze unit was not considered an error, and taking either of the two alternate routes from entrance to exit of a given unit was correct, provided S entered no cul-de-sac. Retracing the entrance (staple) to a block was an error, unless the subject obviously returned to that point as a 'base of operations.' This might be thought too fine a distinction, but in reality there was no difficulty in deciding between the two cases. Retracing to a previous block was prevented by telling the subject that he was 'going backwards.' Errors, time, and trials were recorded for all except the smallest (A) mazes. The latter were not timed because of the difficulty of measuring very short time intervals with sufficient accuracy.

Each part or whole was considered learned when a subject was able to trace three successive errorless trials. As in our earlier work on whole-part learning, the two-part division was used. Each subject learned 38 mazes of four sizes: 2, 4, 8, and 16 units, called A, B, C, and D complexities respectively. The order of learning was AABBCCCCBBAA, BBCCDDDDCCBB, CCCCCC, DDDDDDDD. The mazes were numbered 1 to 38 in the order given, and in subsequent discussions (for example) C 6 refers to an eight block pattern learned in the sixth position, D 33 signifies a sixteen unit maze learned after 32 others had been mastered, and so on. It will be noted that mazes 1–12 and 13–24 are cycles of three complexities in counterbalanced order. I originally intended to use three cycles AABBCCCCBBAA, but at the end of the first cycle two subjects were no longer making errors (except in the first trial) on the *wholes* of the A-mazes; the latter were therefore omitted in subsequent work. Similarly, at the end of the second cycle the 4 block (B) maze no longer offered a sufficiently difficult problem to make comparison of methods of any value. The experiment was therefore concluded with six C-mazes followed by eight mazes of D complexity.

Of the 38 patterns 19 were learned by the whole method and 19 by the part method, in the following order: Subject C, wppwwppwwppw, wppwwppwwppw, wppwwp, pwwppwwp; Subject M, pwwpwppwpwwp, wppwpwwpwppw, pwwppw, wppwwppw. Subject W used the same order as Subject C, except that mazes 7–10 were learned pwwp, and mazes 16–17 pw. Both of these changes were unintended, but in any case the first change made no difference in counterbalancing the order of part

[2] Two screws through each four inch block would be an equally satisfactory but simpler arrangement. A turn of each screw would hold a block in position.

and whole learning for different sizes of maze pattern, and the second involved only one pair of patterns and could only slightly affect the general trend of the results.

For the most part one maze was learned a day. However, when the experiment was about half completed (maze 20) there seemed some doubt whether the desired amount of work could be done in the time available. Since earlier experiments and the general trend of the results showed no evidence of proactive inhibition from the first to the second of two mazes learned in the same day, mazes 21–26 were learned two a day with 5 minute intervals between. However, some signs of proactive inhibition appeared, and although with so few mazes one could not rule out the possibility of a purely chance effect, only one maze was learned on each subsequent day. The two parts of a given maze were of course initially learned and combined in one session.

EXPERIMENTAL DATA

A. First Trial

The earlier finding that factors affecting efficiency of performance on the first trial and in later trials follow different laws (**2**) is supported by the data of the present experiment.

(1) Differences between the relative economy of the part and whole methods on the first trial are negligible when measured by errors, and slight when measured in time. With the eight B (4-unit) mazes the average first trial errors for the part method are 11.1, for the whole method, 11.0; with the 12 C-mazes, part method, 19.5, whole method, 21.2; and with the 12 D-mazes, part method, 41.1, whole method, 41.4. The averages for all 32 B, C, and D patterns, weighted before averaging by multiplying the B values by 4 and the C values by 2, are 41.5 errors for the part method and 42.6 errors for the whole method. With the time measure there is a slight advantage for the part method. The weighted average speed for B, C, and D-mazes is 121.3 seconds for the part method and 131.9 seconds for the whole method. However, this difference is not sufficiently consistent to warrant any definite conclusions, since the whole method is less efficient for only 5 of the 8 groups of mazes.[3] The other 3 show a slight advantage for the whole method.

(2) First trial efficiency is relatively little affected by practice. The error records just given show this to some extent, since on the average B-mazes were learned earlier than C, and C earlier than D patterns. The most valid compari-

[3] For grouping see Table I.

sons, however, are between mazes of the same size. Here the combined results of the part and whole methods give the following averages: A-mazes 1–2, 5.7 errors; 11–12, 3.5 errors; B-mazes 3–4, 14.5 errors; 9–10, 10.3 errors; 13–14, 8.3 errors; 23–24, 11.0 errors; C-mazes 5–8, 20.4 errors; 15–16, 16.3 errors; 21–26, 21.8 errors; 27–30, 20.7 errors; D-mazes, 17–20, 44.1 errors; 31–34, 40.4 errors; 35–38, 39.7 errors. With the C-mazes no practice effect appears, and for the D-mazes the improvement is very small in amount. For the A and B-mazes, however, some positive influence from previous learning is evident. It must be remembered, however, that the A and B values given here represent averages of only two maze records, and are accordingly liable to distortion by chance factors.

The influence of practice on errors and time is so similar that it is not necessary to present detailed time data. The decrease in average number of seconds required for tracing is about 15 percent from mazes C 5–8 to mazes C 27–30, and 20 percent from mazes D 17–18 to D 35–38. Time records for the B-mazes show more irregularity, but still fairly close correspondence with the error measure.

(3) For all three subjects individual differences in average first trial errors are negligible. For average first trial speed, however, this holds only for the two adults. Since practice effects and whole-part differences are small, one comparison can be made for all mazes by combining the data from whole and part learning, and weighting the B and C patterns as previously. The results are: errors, Subject W, 42.2, Subject M, 43.0, Subject C, 40.3; time, Subject W, 110.0 seconds, Subject M, 106.3 seconds, Subject C, 158.4 seconds. The only appreciable individual difference is that C is about 50 percent slower than either W or M. There was some evidence that this difference decreased with practice.

The similarity between W and M in average first trial errors is not remarkable when their method of first trial tracing is considered. After the first few mazes both M and W almost invariably began tracing a given block by trying the first staple to the right or left of the entrance, and then

entering each cul-de-sac in order until they reached the exit. It would be more surprising if such uniformity of method did *not* lead to close correspondence in results. But C's error record calls for a different explanation, since C (apparently more from a taste for variety than from inability to grasp the principle) followed quite a different procedure.[4] The first path in any given case 'looked' to be wholly a matter of chance, though I have reason to believe from her occasional spontaneous introspective reports that she was often following a 'hunch.' If the hunch was wrong and the path proved a cul-de-sac, other paths were tried apparently at random, except that *she almost never entered a given cul-de-sac twice in the same trial.* Evidently there are factors making for uniformity in number of first trial errors other than logical method. The whole subject of first trial errors merits careful analysis, which I hope to attempt in another place.

The additional fact should be noted, however, that there are fewer errors than the method used by W and M would lead us to expect. On the average, one would anticipate that taking the right or left hand route at random would lead to a subject making just one-half of the possible (6) errors, or 3 errors per unit. Since our weighted data are based on the 16-unit mazes, the average first trial errors for all mazes should be 48. Yet W's average is 42.2 and M's average is 43.0. The most plausible guess regarding the cause of this discrepancy is that (in order to keep all parts of each path within a subject's reach) I inadvertantly made all or most patterns with a 'tendency' to turn more often in one direction than the other. A grasp of this direction early in the run could thus aid a subject to choose the shorter route over a given block with better than chance expectation.

(4) There are no significant differences in the relative ease of tracing part I and part II. The average errors (weighted as above) for the B, C, and D-mazes are 41.8 for the first half, and 43.2 for the second half of the patterns.

(5) The difficulty of first trial tracing is almost directly

[4] Occasionally while tracing one or another of the last few mazes C used the same method as M and W.

proportional to size of maze pattern. Since practice and whole-part differences are small, we may average the results of all A, all B, all C, and all D-mazes. These averages are: errors, A, 4.8; B, 11.1; C, 20.3; D, 41.3; time, B, 36.3; C, 58.3; D, 116.6 seconds. The errors per unit are 2.4, 2.8, 2.5, and 2.6 for the A, B, C, and D patterns, and the time per unit for the last three 8.8, 7.3, and 7.3 seconds. Here the only deviation from direct correspondence between size and difficulty is the 8.8 seconds per unit for the B patterns.

B. Later Trials

1. *Whole versus Part Learning*

For trials other than the first the average errors, time, and trials are given in Table I. The mazes are grouped

TABLE I

Trials Other than the First. Time in Seconds

Group	Maze No.†	Errors			Time			Trials		
		Part	Whole	Part Adv.*	Part	Whole	Part Adv.*	Part	Whole	Part Adv.*
I.....	A 1–12	1.9	3.8	100				5.4	2.7	−100
II.....	B 3–10	7.5	8.2	10	118.2	74.5	−59	6.7	4.2	−60
III.....	B 13–24	2.3	2.2	−5	67.0	26.0	−158	5.4	2.2	−150
IV.....	C 5–8	33.3	44.7	34	460.5	476.3	4	14.8	12.3	−20
V.....	C 15–26	15.4	27.3	77	306.9	229.6	−34	9.6	6.7	−43
VI.....	C 27–30	7.0	16.7	139	159.5	166.8	5	5.7	5.0	−14
VII.....	D 17–20	50.2	154.5	208	818.0	1118.0	44	12.7	13.0	2
VIII.....	D 31–38	29.0	61.0	110	607.7	562.2	−8	9.9	8.0	−24

* Percent advantage of the part method over the whole method. Negative numbers indicate that the whole method is superior.

† Numbers indicate the mazes of a particular complexity that were learned between those limits. Thus the A-mazes were numbers 1, 2, 11, 12.

according to size of pattern (A, B, C, D) and order of learning. Only mazes of the same size are included in any one group. The mazes in each group were for the most part learned in immediate succession. The exceptions to this rule may be found by examination of the order in which the A, B, C, and D patterns were learned, as given in the description of procedure. Each group comprises four patterns except C 15–26 (6), and D 31–38 (8). The 'percent advantage of

part over whole' was computed as in the two previous investigations. Where the whole method is superior these values are negative.

Table I shows that fewer errors are made with the part method and fewer trials are required with the whole method. Only one exception is found to either generalization, and neither is large enough (5 percent advantage in errors for whole learning with Group III, and 2 percent advantage in trials for part learning with Group VII) to require separate consideration. The time measures in general indicate a superiority for whole learning. For three groups of mazes the whole method requires markedly less time, three show little advantage for either method, and one group (VII) gives a 44 percent superiority for the part method.

The factors conditioning the relative efficiency of the three measures in whole and part learning have been discussed in earlier papers (**1**, **2**). The superiority of the whole method by the trial criterion in the present experiment seems largely explicable by the three errorless trials for initial part learning which are included in the total trials for the part method. These play a large part in the present instance because the total number of trials is small. In every case except Group III, for which all measures favor the whole method, subtracting three trials from the part method totals would give lower values than the corresponding totals for the whole method. The relatively small number of errors as compared with trials required for combining the parts was also in evidence in the present experiment. The errors made in combining the parts comprised the following proportions of the total number of errors: 25, 45, 30, 30, 39, 3, 10, and 5 percent for Groups I to VIII respectively. The corresponding trial ratios (omitting the first trial and the three final trials for initial part learning) [5] are: 57, 48, 50, 55, 77, 12, 43, 24 percent. With every group relatively more trials than errors are made in combining the parts.

[5] This was not done by Cook, Morrison, and Stacey because in their experiment the total number of trials was so large that the inclusion or omission of these four trials make little or no difference in their percentage values. In our case the small number of total trials forbids this.

The same factors also account in some measure for the inferiority of the part method in total time. Some additional explanation is needed, however, for the large discrepancy between the time and error data. With D 17–20, for example, the part method is 208 percent superior in errors and only 44 percent superior in time. For the visual maze the greatest superiority of the part method was 166 percent in errors and 72 percent in time. To a slight degree the greater difference between the time and error advantage in the present experiment may be attributed to the fact that each trial in combining the parts consumed a relatively much greater time than in the visual maze. The chief factor, however, is probably that those unique features of the spider maze designed to penalize lack of insight and inability to remember the pattern bore more heavily on errors, and to a much less degree on time. The visual maze has two cul-de-sacs at each choice point; the spider maze has six, thus increasing the chances for error and the number of errors when chance plays an important part in entry into the correct path, and conversely placing a heavy 'error premium' upon memory of the correct path. But the blind alleys are shorter and the junctions between units much longer in the spider maze, so that each error carries a relatively lighter penalty in loss of time.

It should be noted that the wide range of percentages appearing in Table I do not signify chance variation, and have little or no bearing on the reliability of the data. The 'spread' is the result of factors varying for the most part in certain constant directions, and more or less adequately analyzed in the subsequent discussion.

2. *Amount of Material*

a. Relative Difficulty of Part I and Part II

For the part method differences in relative difficulty of part I and part II are insignificant. The average errors are 1.0, 1.9, 6.7, and 17.3 for the first half and 1.2, 1.2, 5.7, and 16.3 for the second half of the A, B, C, and D-mazes. The .7 difference with the B-mazes is relatively large, but examination of the records shows that it depends almost entirely

upon the data from two subjects from mazes B 3 and B 4. It may be that the slightly lower averages for the second half of the maze patterns (except A) indicate a practice or warming-up effect. None of the differences, however, are large enough to markedly affect the reliability of computations of the relation between length of maze pattern and difficulty of learning.

For the whole method the average errors in part I were 2.0, 2.6, 12.6, and 36.4, and in part II 1.4, 2.6, 15.6, and 55.3 for the A, B, C, and D patterns. Of these only the values for the D patterns (36.5 and 55.3) are sufficiently large to merit discussion. But as differences between part I and part II in whole learning are only of importance when we attempt to discover *why* difficulty increases with amount of material, we will defer detailed discussion until another time.

b. Relation of the Whole-part Problem to Size of Maze Pattern

(1) *Initial Learning of the Parts*

Table II indicates that increase in difficulty is greater than the increase in size of maze pattern. This has been shown

TABLE II

LENGTH AND DIFFICULTY. TIME IN SECONDS

Group	Maze No.	Errors			Time			Trials		
		Part	Whole	Ratio	Part	Whole	Ratio	Part	Whole	Ratio
I.....	A 1–12	.7	3.8	5.4				.5	1.7	3.4
II.....	B 3–10	2.1	8.2	3.9	22.2	74.5	3.4	1.3	3.2	2.5
III.....	B 13–24	0.9	2.2	2.4	7.5	26.0	3.5	.7	1.2	1.7
IV.....	C 5–8	10.8	44.7	4.1	94.4	476.3	5.0	4.7	11.3	2.4
V.....	C 15–26	4.7	27.3	5.8	46.1	229.6	5.0	2.0	4.7	2.8
VI.....	C 27–30	3.4	16.7	4.9	38.2	116.8	4.4	1.2	4.0	3.3
VII.....	D 17–20	22.6	154.5	6.8	217.6	1118.0	5.1	5.4	12.0	2.2
VIII.....	D 31–38	13.8	61.0	4.4	174.5	562.1	3.2	4.6	7.0	1.5

previously to be the chief factor favoring the part method; the present experiment confirms the earlier finding. For A and B-mazes between 1 and 12 the practice effect is too great, and individual differences are too large for the results to be more than suggestive, but for mazes C 5–8 and for mazes 13–38 (B, C, D) a comparison of Table I and Table II shows

that in errors the percent advantage for the part method is roughly proportional to the rate of increase in difficulty of initial learning of the parts as compared with the wholes. The only striking exception is the 139 percent superiority with mazes C 27–30, where the wholes are only '4.9 times as difficult' as the parts. The cause of the exception is that at this stage the improvement from practice has been more marked for combining the parts than for initial learning. In fact only one error was made in 6 attempts at combining mazes 27–30.

It further appears that the increase in difficulty is greater for errors than for time, and for time than for trials. The average increase in difficulty from parts to wholes of the A, B, C, and D mazes is 4.6 for errors, 4.1 for time, and 2.2 for trials. This indicates that part of the difference in the relative efficiency of the three measures in part and whole learning (errors favoring the part method and trials the whole method) appears in the initial learning.

In an earlier paper (**2**, Fig. 1) I presented a sample curve showing the course of error deletion trial by trial for parts and wholes of the 24-unit visual maze patterns. Curves constructed by a similar treatment of the results of the present experiment show the same general characteristics. Some differences of a minor character appear, but I have not yet gathered enough data to decide whether these are due to dissimilarity between the problems presented by the visual maze and the spider maze, or to individual peculiarities of the subjects.

In that study it was also found that the Kjerstadt-Robinson Law does not hold for visual maze learning. My results for the spider maze show a like divergence from the law. Fuller discussion will be deferred until further experiments have been performed.

(2) *Combining the Parts*

The average errors made in combining the parts were 0.5 (A), 3.5, 0.7 (B), 11.8, 6.0, 0.2 (C), 4.8, 1.5 (D) for Groups I to VIII. The improvement from practice is so great that very few errors are made in combining the last learned B,

C, or D patterns. The percentages of the total errors for the part method made in the act of integrating the parts, however, were (as given previously) 25, 43, 30, 30, 39, 3, 10, and 5, indicating that the combining errors make up roughly one-third of the part totals with the first five groups of mazes. Thereafter combining has profited so much more than initial learning from practice that the former make up a very small proportion of the gross number of errors for part learning.

3. *Practice*

Improvement from practice is relatively large and is continuous throughout the experiment. With the whole method, which is the most convenient standard of comparison for mazes of the same size, B 13–24, C 28–30, and D 31–38 were traced with 25, 35, and 40 percent of the errors made in B 3–10, C 5–8, and D 17–20 respectively. For time and trials the practice effect is striking and equally consistent, though less for time than for errors and less for trials than for time.

The relation of practice to the relative economy of part and whole learning is not so easy to determine. That it operates in some such fashion as was suggested in earlier papers is evident, but the divergence between rate of improvement in initial learning and in combining the parts, and the effect of individual differences (discussed below) makes its precise role more difficult to discover. Yet several things are clear. (1) Practice in combining the parts favors the part method. The latest learned mazes of each size were combined with very few errors. (2) Whether practice in initial learning favors the part method or the whole method is a function of the location of the 'most economical unit' for a given stage of individual efficiency. Previous studies have indicated that at some point in the scale of increase in size of pattern there is a sharp rise in difficulty, and that the location of this 'critical point' changes with practice, the subject becoming able to handle larger amounts of material with maximum efficiency. In the main this generalization is supported by the data from the present experiment. For the earlier B mazes (3–10) the increase in difficulty is 3.9 from the 2-unit parts to the 4-unit

wholes; for the second group of B mazes (13–24) the corresponding increase in difficulty is 2.5. For the C patterns the difficulty ratio first rises and then falls. The increases are: C 5–8, 4.1 times; C 15–26, 5.8 times; C 27–30, 4.8 times. It would seem that for the C mazes an intermediate level of practice shows the sharpest rise in the difficulty curve. With the D-mazes the level of practice represented by D 17–20 shows the greatest influence from the initial difficulty factor: the parts are 6.7 times as difficult as the wholes. But with D 31–38 practice has so increased the efficiency of 16–unit learning that only 4.4 times as many errors are made in the wholes as in each of the 8-unit parts. It will be noted that at no time is the increase in difficulty with the B or C-mazes as great as that for the D-mazes (6.7 times). This would seem to indicate that for well practiced subjects the 8 to 16-unit jump involves the greatest spread in difficulty. (3) For time and trials the three errorless trials taken as the standard of 'perfect' learning, and counted in the measures for part learning, played an increasingly large part as practice decreased the number of trials or amount of time required for initial learning. This factor accounts in some measure for the increasing superiority of the whole method with practice, when superiority is measured in time or trials.

4. *Individual Differences*

Large differences between the performances of the three subjects undoubtedly add to the difficulty of discovering the precise part played by the other variables. At the same time the individual results are highly suggestive, and admit some probable if tentative conclusions. Two chief differences appeared. (*a*) Subjects differed in the size of the unit that they were able to handle with maximum efficiency. This factor was effective chiefly during initial learning of the parts, and, especially when combined with the influence of practice, tended to obscure the distinction between the efficiency of the part and whole methods. (*b*) The second main difference was in resistance to upset by 'accidental' internal or external influences. Such upsets occurred more often in the adjustment phase of learning, and thus worked against the part

method and in favor of the whole method, since the former required three initial adjustments to one for the latter.

(1) Subject M

Subject M was the most consistent performer of the three; consequently, although he was not the most rapid learner his results are best considered first. The most remarkable general feature of M's data is the consistent superiority of the part method. Except for the A-mazes (which were so easy that a chance variation could readily throw the balance one way or another) all error measures favor the part method. Also, except for B 13–14 (M made no errors on B 23–24 by either method of learning) in which M made only 3 errors with the part method and 4 errors by the whole method, and except for a slight advantage of the whole method with D 17–20, all time measures show the part method superior. Finally, M's trial values, except for the later B-mazes (which were traced with practically no errors after the first trial), show no consistent advantage for either method. The averages are 8.9 trials for part learning and 9.1 trials for whole learning.

The superiority of the part method with Subject M is almost certainly to be attributed to a cool and businesslike approach, complete concentration on the task in hand, and high resistance to accidental disturbing influences. Though not so rapid a learner as W he was a far more consistent learner. Except for practice the range of variation in his records is small. Moreover, he made few errors in combining the parts: only 43 in all, as contrasted with 77 for C and 80 for W. Of these 43 errors, 35 were made in combining C 5–8 (18) and D 17–20 (17), and are not due to chance variation, but to a genuine difficulty in combining the parts at a particular stage of practice. After learning ten more mazes, M combined four of the D 31–38 group (VIII) with only two errors, an average of .5 per maze.

M's results also demonstrate in an interesting way the importance of the most economical unit and its relation to practice, although care must be taken not to infer too much from the separate results for each group of mazes. The error measure is best for illustrative purposes, since it is not

distorted by the disproportionate influence upon the smaller mazes of the three final trials in initial part learning.

For B 3–10 the superiority of the part method in errors is 183 percent; for B 13–14, only 33 percent. C 5–8, which were learned between B 3–4 and 9–10, show 63 percent part advantage. Evidently with Subject M the 2-unit parts are most economical at the 3–10 level of practice. However, the 2-unit parts are soon displaced from their favored position. C 15–16 (4-unit parts) show 396 percent advantage and C 27–30, 418 percent advantage for part learning. On the other hand, the 8-unit parts were clearly too difficult to give maximum economy with part learning, yielding only 107 percent superiority for D 17–20, and 102 percent superiority for D 31–38.

(2) Subject W

W learned most easily and rapidly of all subjects. At first, however, she did not like the part method. The triple adjustment disturbed her, and in order to be sure that she would remember part I until combining was called for, formed a 'habit' of going over part I while learning part II. This probably caused some inhibition, both proactive and retroactive. By the time these tendencies were overcome, W had become so proficient that the wholes of subsequent B or C-mazes came below or within her 'most economical unit.' Thus the average for the five C-mazes learned (by the whole method) between mazes 15 and 30 was 3.7 errors; M made an average of 37.2 errors on the same mazes. Accordingly W's results for all the B-mazes and for the C-mazes up to 27–30 show the whole method superior by all measures. For C 27–30 the part method is slightly superior: 63 percent in errors, 80 percent in time, and 10 percent in trials. But the data from mazes D 17–20 show clearly that (at this level of proficiency) the 8-unit parts are most economical. Here the advantage of the part method is 580 percent in errors, 126 percent in time, and 60 percent in trials. With D 31–38 the efficiency of the part method begins to decline. For errors the part superiority is 386 percent, while for time and trials the whole method has the advantage. This shift is wholly

due to a rapid drop in the difficulty of the 16-unit wholes (from 115.5 average errors with mazes 17–20 to 40.0 average errors with mazes 31–38) without a corresponding gain in the parts (17.0 to 8.8). Combining the parts offers little difficulty with either group of mazes: the average errors per maze is 2.0 with D 17–20 and 2.5 with D 31–38.

(3) Subject C

Subject C showed the greatest variability and made the poorest average records of any subject, in spite of an occasional performance better than M and equal to W. The variability seemed largely caused by 'accidental' influences, and occurred chiefly during the adjustment stage of learning, thus favoring the method (whole) requiring but one adjustment. Accordingly C's data show fewer instances of superiority with the part method and more instances of advantage for the whole method than either of the other subjects. In errors the A-mazes favored the part method and the first six B-mazes the whole method. For all other mazes C made fewer errors with the part method, but with one exception the difference between the efficiency of the two methods is small, ranging from 30 percent with C 27–30 to 69 percent with D 31–38. C 5–8 and D 17–20 (30 and 37 percent) were the only groups of mazes for which the part method was more economical of time. Subject C also required more trials with the part method for all maze groups except C 5–8. The exception to the general trend of the error records is D 17–20, with 201 percent advantage for the part method.

Subject C's variability with the part method makes it difficult to analyze her data with respect to the roles of practice and the most economical unit. The only obvious trend is a steady decrease in the difficulty of combining the parts. The average errors made in combining were: B-mazes, Group II, 6.0, III, 2.0; C-mazes, Group IV, 10.0, V, 8.3, VI, 0.0; D-mazes, Group VII, 4.0, VIII, 1.5. Each successive group of mazes of like size is less difficult to integrate, and for the latest learned groups of B, C, and D-mazes (III, VI, and VIII) the number of errors is in no case appreciably greater than zero. From this trend alone it might be anticipated

that C 27–30 and D 31–38 would show a considerable advantage for part learning in number of errors. That they did not is due to the fact that the 'wholes' of the C-mazes 27–30 were learned with an average of 15.0 errors per maze, while the parts of mazes D 31–38 (though the same size as the C wholes) show an average error of 26.5. Examination of the records for 8-unit learning for all three subjects, however, would indicate that this is a 'chance' variation. The 15.0 value for mazes 27–30 is much below the preceding and succeeding 8-unit averages for Subject C. The five groups of mazes in which 8-unit learning occurs (wholes in C and parts in D mazes), IV to VIII, were learned with the following average errors (to the nearest whole number): Subject M, 41, 42, 28, 26, 22; Subject W, 36, 4, 6, 7, 6; Subject C, 56, 32, 15, 33, 26. The variation is not due to maze differences, since it occurs only with Subject C.

Our analysis of the results of the three subjects suggests that individual differences make the distinction between the relative economy of the part and whole methods for various sizes of maze pattern and stages of practice less clear-cut than would be the case for subjects of equal ability. It must of course be remembered that the order of learning was counterbalanced for M on the one hand and W and C on the other, and that part of the apparently individual differences may be due to variation in difficulty of maze pattern. It does seem clear, however, that W's most economical 'part' was 8 units and M's 4 units. Her only records showing a reliable advantage for the part method in errors were 580 percent for D 17–21, and 354 percent for D 31–38. Subject M, on the other hand, found the part method only 107 and 102 percent superior for the two groups of D-mazes, but 396 and 418 percent superior for C 15–26 and C 27–30. In both of these cases a sufficient number of mazes were learned (10 C and 12 D-mazes) to 'average out' differences in difficulty of maze patterns. The hypothesis that best agrees with the evidence is that here we are dealing with a genuine individual difference.

4. Recency and Retroactive Inhibition

In previous work (2) I found some evidence that during

the act of integration more errors were made in part I than in part II. The present experiment shows a like result. The total number of errors made in all mazes during the *first* combining trial was 75 for part I and 21 for part II. As previously pointed out, this might be attributed either to the greater recency value of part II (since it was last learned) or to retroactive inhibition of part I by the interpolated learning of part II.

SUMMARY

The whole-part problem was investigated with finger mazes composed of standard units having entrance, exit, and 6 culs-de-sac in each unit. Thirty-eight patterns of four sizes (2, 4, 8, and 16 units) were learned by each of 3 subjects, 19 patterns by the whole method and 19 by the part method. The results differ for the first trial and for later trials.

A. First Trial

For the error measure first trial differences between the relative economy of part and whole learning, individual efficiency, and relative difficulty of part I and part II are negligible, practice effects are slight, and difficulty is proportional to size of maze pattern. The time measure shows like results except that one subject required about 50 percent more average time for tracing than either of the other two.

B. Later Trials

(1) The part method is in general superior in errors, and the whole method in trials and time; but any large advantage in errors is accompanied by a small or moderate superiority for the part method in time.

(2) Disproportionate difficulty of initially learning the larger amounts of material favored the part method. For trials difficulty is proportional to size of pattern, for time and errors difficulty increases on the average as the square of the size of pattern.

(3) The relation of practice to the relative economy of the part and whole methods in initial learning is a function of the location of the 'most economical unit.' For amounts of

material smaller than this unit practice favors the whole method, for larger amounts of material, the part method.

(4) A larger percentage of (the total) trials than errors were made in combining the parts. For 8 unit and 16 unit mazes these percentages decreased with practice. In this respect practice favored the part method.

(5) The subject for whom the part method was most advantageous showed the greatest part superiority with the C-mazes, where he made four times as many errors by the whole method. A second subject found the whole method superior with the smaller patterns, but made five times as many errors by the whole method with the 16 unit mazes. The third subject followed the average trend, but with more than average advantage for the whole method in time and trials, and less advantage for the part method in errors. The evidence suggests that the most significant individual differences are the size of the 'most economical unit' of material, and degree of resistance to distraction.

(Manuscript received October 19, 1936)

References

1. Cook, T. W., Morrison, S. H., and Stacey, C. L., Whole and part learning of a visually perceived maze, *J. Genet. Psychol.*, 1935, 47, 218–232.
2. Cook, T. W., Factors in whole and part learning a visually perceived maze, *J. Genet. Psychol.*, 1936, 49, 3–32.

AN EXPERIMENTAL STUDY OF THE LURIA TECHNIQUE FOR DETECTING MENTAL CONFLICT[1]

BY JOHN W. GARDNER

Stanford University

Review of the Literature

In *The Nature of Human Conflict* (**15**) Luria describes a technique which he has used successfully over a period of years in the detection of guilt, and in systematic studies of emotion and the disorganization of behavior. The subject is required to place the fingers of his two hands upon a pair of sensitive tambours, and during the administration of the free word association test every slight variation of his manual pressure upon these tambours is recorded. At the same moment that the subject gives his free-associative response he is required to depress the tambour which is governed by his preferred hand. He is directed to hold his other hand steady throughout the experiment. According to Luria certain disturbances in the pressure records during the test are diagnostic of emotional disorganization or mental conflict on the part of the subject. He does not attempt quantitative measurement of these disturbances in the pressure records.

Olson and Jones (**16**) made use of this technique described by Luria in an attempt to measure the intensity of attitudes towards certain controversial issues, and their results indicate that this procedure may be successfully applied in that field. They made quantitative measurements of one of the 'motor symptoms' described by Luria, and evaluated their results statistically.

[1] Condensed from a thesis written under the direction of Dr. E. R. Hilgard, and accepted by the Department of Psychology of Stanford University in partial fulfillment of the requirements for the degree of Master of Arts. The writer is indebted to Mr. Milton L. Phillips for invaluable aid in the devising of the apparatus, and to Dr. Albert A. Campbell for criticisms and suggestions.

In 1934 Huston, Shakow and Erickson (9) repeated one of Luria's experiments which involved the induction of a conflict in the subject by means of hypnosis. Free-association tests were administered in which the crucial words referred to this hypnotically induced conflict. The conflict was successfully induced in 9 of the 12 subjects, and of these only 6 showed 'motor symptoms' of the sort reported by Luria. In view of the small number of subjects statistical treatment was not attempted.

Barnacle, Ebaugh and Lemere (1) published in 1935 the results of an experiment involving the Luria technique. Using psychoneurotic subjects with crucial words taken from the individual case histories, they succeeded in obtaining the sort of results described by Luria. They do not state how the motor disturbances were measured, or how they determined whether or not a response was disturbed.

Burtt (2) has recently employed the Luria technique in an experiment "involving the determination of which of two boxes a subject had inspected." He analyzed the pressure curves qualitatively, and noted that quantitatively the most diagnostic feature was the time by which the subject's manual response tended to anticipate his verbal response to the stimulus word (the two are supposed to coincide).

Statement of the Problem

Inasmuch as previous work with the Luria technique has been predominantly qualitative, it was thought desirable to determine to what extent the presumably diagnostic 'motor symptoms' would be validated by quantitative treatment. With this objective in mind an experiment was set up to test the technique; a simple scheme for measuring the motor disturbances was worked out, and the results were evaluated statistically.

Methodology

1. *Stimuli.*—Fifty words were presented serially by means of an electrically-controlled exposure apparatus to each of a group of 33 male students. Following is the list of words in the order of their appearance. The 'critical' words are italicized.

Light	Walk	Yellow
Carpet	*Rape*	Window
High	Ocean	Music
Slow	Glass	*Moral*
Salt	*Desire*	River
Sex	Bitter	*Dream*
Black	Work	Heavy
Lamp	*Ravish*	Tree
Green	Tobacco	*Shame*
Embrace	*Kiss*	Loud
Scissors	Sour	Thirsty
Caress	King	House
Blossom	Butter	*Body*
Hat	Garden	Swift
Prostitute	*Fondle*	*Sin*
Eagle	Lion	Bread
Cottage	*Worry*	

The 'critical' words total fifteen. Luria's 'critical' words were designed to perform one or both of the following functions; either to tap some affective complex, *i.e.*, to arouse emotion in the subject, or to cause the subject to avoid responding with the first word which came to his mind. Probably in most cases these functions are inseparable; but in all cases one or the other condition prevailed. We chose our critical words in such a way that they would be very likely to perform the second function; that is, we felt reasonably sure that as a rule the subject would not be completely willing to respond to them with the first word which came to his mind. These words were for the most part associated with sex. This scheme of employing words with a sexual connotation has been used with success by Crosland and Beck (4).

The remaining thirty-five words were chosen from the neutral words on Crosland's list, and from the words on the Kent-Rosanoff list which showed the greatest uniformity of response according to the frequency tables standardized at Stanford University,[2] ruling out those words which were thought to have possible sexual connotations. These words make up the Neutral and Post-critical groups in the following table.

The critical words were scattered irregularly throughout the list. No two critical words were adjacent, and the last half of the list contained as many of these words as did the first half.

2. *Apparatus.*[3]—The apparatus consisted of four main parts: (1) an electrically-driven exposure apparatus, (2) a pair of levers upon which the subject placed his fingers (replacing the tambours of Luria), (3) an electrically-driven camera to record photographically the manual pressures as transmitted from the above-mentioned levers over a cord-and-pulley system to a pair of recorders in front of the camera, and (4) a spoked wheel which produced time-lines upon the photographic record at half-second intervals. A signal marker connected with the exposure apparatus operated to record the instant at which each word was exposed to the subject's view, and a signal marker operated by the experimenter recorded the moment of the subject's response.

[2] These frequency tables are based upon results from 500 college students tested by C. Mansel Keene in the course of a doctoral investigation as yet unpublished.

[3] In the interest of brevity a detailed description of the apparatus is omitted. A more complete account is to be found in the original paper (6) on file at the Stanford University library.

3. *Subjects.*—The thirty-three male subjects from whom records were obtained were students in an elementary psychology class, and were presumably a normal group of college students. Since the subjects were run at the beginning of the term, it is probable that they were not acquainted with the principles of diagnostic free-association, and it is quite certain that they were unaware of the significance of the levers.

4. *Measurement of the Records.*—Measurements were taken of verbal reaction time, and of three Luria symptoms—disturbances in the record of the preferred hand during the latent period, perseverative pressures in the preferred hand, and disturbances in the record of the non-preferred hand.

(*a*) *Verbal Reaction Time.*—As has been stated, a spoked wheel produced time-lines on the records at half-second intervals, so the measurement of reaction time became a comparatively simple matter. The half-second lines were far enough apart so that it was possible to measure to the nearest quarter of a second.

(*b*) *Disturbances in the Record of the Preferred Hand During the Latent Period.*—Luria required the subject to respond with a pressure of his preferred hand at the same moment that he responded verbally. He found that after a very brief practice period this voluntary movement became an integral part of the subject's response to the stimulus, with the important result that when the subject had a *tendency to respond verbally* he also had a *tendency to respond manually*. In short, the manual pressure is brought in as an additional avenue of response, and the subject accepts it as such. The value to the technique of this manual pressure lies in the fact that whereas the subject apparently finds comparatively litttle difficulty in repressing the tendency to respond verbally (at least to the extent that the experimenter is unaware of his repression), he is not so successful in repressing this tendency on the manual level,—first because the slightest movement upon this level is caught by the sensitive levers, and secondly because he is not devoting his attention to this response, *i.e.*, he is not aware that he is betraying himself on this level. The result would be, then, that if the subject repressed a tendency to respond with the first word which came to his mind, this repressed tendency would reveal itself in the form of a slight deflection in the pressure record preceding the final response he gives when he has found a word which he considers more suitable.

In order to determine the extent to which this symptom appeared, all deflections occurring between the moment when the word-stimulus appeared and the moment when the subject gave the required motor response were measured in millimeters. For each such interval the *range of deflection*, taken from the lowest point to the highest point of this section of the curve, was measured and recorded as the score for that interval.

(*c*) *Perseverative Pressures in the Record of the Preferred Hand.*—It was noted by Luria that excitation aroused by certain stimulus-words was so excessive that it could not be entirely released through the simple required response, and tended to cause perseverative pressures after the required response had been executed. In order to measure this particular motor symptom, all the deflections occurring after the required response of the preferred hand and before the appearance of the next word were taken into account. Here, too, the range of deflection was measured in millimeters.

(*d*) *Disturbances in the Record of the Non-preferred Hand.*—The non-preferred hand was expected to maintain a slight, steady pressure upon its lever, and under normal conditions this resulted in a fairly smooth, level pressure line with only small variations, according to Luria. But in the event of 'emotional disorganization' the smoothness of this pattern was broken, and irrelevant pressures appeared. In order to study the oc-

currence of this symptom, all deflections occurring in the record of the non-preferred hand from the appearance of the word-stimulus until the appearance of the succeeding stimulus were taken into account.

RESULTS

Each of the four measures discussed above is given separate treatment in order to determine its significance in detecting differences between critical and non-critical words.

1. *Verbal Reaction Time.*—The verbal reaction times for all the word-stimuli were obtained from the records as described above. Then from these data three means were calculated for each subject,—a mean reaction time to critical words, a mean for post-critical words, and a mean for neutral words. It was found that thirty-one out of thirty-three subjects showed a longer mean reaction time to the critical words than to the neutral words. In the final statistical calculation, group means for each of the three classes of word-stimuli were secured from these mean scores for individual subjects. Thus the average of the individual means for the neutral words was 1.85″; the average figure for the critical words 2.81″. In order to determine the significance of the difference between these values, the standard deviation of the difference was calculated and the critical ratio (the difference divided by the standard error of the difference) was determined. Taking into account the correlation between the two variables ($r = +.35 \pm .11$) the critical ratio was found to be 4.68, indicating that the difference obtained was thoroughly reliable from a statistical point of view. Complete results for verbal reaction time are presented in Table I.

The mean value for the neutral word-group, 1.85″, compares favorably with the 2.0″ quoted by Wells (18) as 'a general mean value for free-association time,' and with the 1.8″ reported by Jung (10).

Contrary to the findings of previous investigators the post-critical words did not yield a lengthened reaction time. This might have been due to the mildness of our word-stimuli or to the emotional stability of our particular subjects. More probably it is due to the fact that six seconds elapsed between the time the subject responded to one word and the time the

TABLE I

VERBAL REACTION TIME

$N = 33$

Word Group	Mean	Standard Error of the Mean	
Critical	2.81 secs.	±.22	
Post-critical	1.68	±.05	
Neutral	1.85	±.06	
Difference between means of critical and neutral words	.97	±.21	Critical Ratio..4.68

Intercorrelations

	r	PE_r
Critical vs. Post-critical	+.53	±.09
Critical vs. Neutral	+.35	±.11
Post-critical vs. Neutral	+.76	±.05

Note: (1) As explained above, the means in the table represent distributions of individual means. (2) In obtaining the standard error of the difference the correlation between the two variables was taken into account.

next word appeared—probably a somewhat longer interval than that usually employed.[4]

The essential point brought out by these reaction time data, however, is the evidence that the subjects showed consistently longer reaction times to the critical words. Lengthened verbal reaction time is one of the classical indicators of the presence of an affective complex or conflict. Jung (10), dealing with psychoneurotic subjects, found lengthened reaction times in connection with words which were selected for their value in tapping complexes of such subjects; and, dealing with criminals, he found longer reaction times to words having some connection with the crime. Crosland and Beck (4), dealing not with psychoneurotics or criminals but with college students chosen at random, employed as critical stimuli words of a sexual nature, and found the same slow reactions. Luria noted the same phenomena in his experiments. Practically every other investigator in the

[4] We cannot state this positively because previous investigators do not report the length of this interval. Our only clue to the usual length of the interval is the emphasis which a number of investigators place upon a 'rapid-fire' presentation of the stimuli. Our method of presentation could hardly be characterized as 'rapid-fire.'

field has noted these lengthened reaction times, and has attributed them either to the presence of emotion, which disrupted the response, or simply to the fact that the subject refused to respond with the first association which came to his mind. As we have already stated our critical words were chosen with the hope that the subjects would react to them in the latter fashion. The results provide reasonable assurance that we were successful on this point; the lengthened reaction times indicate that the critical words were satisfactorily fulfilling the function for which we selected them.

2. *Disturbances in the Record of the Preferred Hand During the Latent Period.*—Theoretically, if the conflict produced by the critical words was strong enough to produce lengthened reaction times it should also produce motor symptoms of the type described by Luria, since Luria attributed both lengthened reaction times and motor disturbances to the same underlying factor, namely the presence of inhibited impulses and disorganization arising from the conflict. The objective measurement of these motor disturbances is the chief concern of this investigation.

Of the various motor symptoms described by Luria, greatest stress is laid upon those disturbances occurring in the record of the preferred hand during the latent period. As has been explained above, repressed tendencies to respond are apt to reveal themselves in the pressure record in the form of preliminary pressures appearing in the interval between the time when the stimulus-word is exposed and the time when the required pressure is finally given. The group results on this measure were obtained in the same manner as were the results on verbal reaction time; for each individual the mean score on each word-group was calculated, and entered in the table. The complete results are presented in Table II.

The difference between the mean deflection for the critical words and the mean for the neutral words is statistically significant, with a critical ratio of 4.16. The direction of the difference and the fact that it is statistically reliable indicates that there is a consistent tendency for such disturbances as that described above to appear in connection with the critical

TABLE II

DISTURBANCES IN THE RECORD OF THE PREFERRED HAND DURING THE LATENT PERIOD

$N = 33$

Word Group	Mean	Standard Error of the Mean	
Critical	2.49 mm.	±.36	
Post-critical	1.94	±.35	
Neutral	1.65	±.31	
Difference between means of critical and neutral words	.84	±.20	Critical Ratio. . 4.16

Intercorrelations

	r	PE_r
Critical vs. Post-critical	+.88	±.03
Critical vs. Neutral	+.83	±.04
Post-critical vs. Neutral	+.93	±.02

words on the test. Again the post-critical words fail to show particularly striking symptoms of perseveration; although the mean for the post-critical group is somewhat higher than the mean for the neutral word-group, the difference is not statistically reliable.

3. *Perseverative Pressures in the Record of the Preferred Hand.*—A second type of motor symptom described by Luria is the appearance of perseverative pressures in the record of the preferred hand. The results of the measurement of this symptom are given in Table III. As with the previous

TABLE III

PERSEVERATIVE PRESSURES IN THE RECORD OF THE PREFERRED HAND

$N = 33$

Word Group	Mean	Standard Error of the Mean	
Critical	6.29 mm.	±.76	
Post-critical	5.21	±.58	
Neutral	5.30	±.63	
Difference between means of critical and neutral words	.99	±.36	Critical Ratio. . 2.71

Intercorrelations

	r	PE_r
Critical vs. Post-critical	+.92	±.02
Critical vs. Neutral	+.88	±.03
Post-critical vs. Neutral	+.94	±.01

measures, these results are in the expected direction; *i.e.* there are more motor disturbances in the voluntary hand following the critical words than there are following neutral words. However in this instance the difference between them does not appear to be statistically significant, the critical ratio falling somewhat short of the desired figure.

4. *Disturbances in the Record of the Non-preferred Hand.*— The last of the motor symptoms measured was the disturbance in the record of the non-preferred hand. Table IV reveals that while the results for this measure were in the expected direction, they are not up to the standard of statistical reliability.

TABLE IV

DISTURBANCES IN THE RECORD OF THE NON-PREFERRED HAND

$N = 33$

Word Group	Mean	Standard Error of the Mean	
Critical	2.35 mm.	±.33	
Post-critical	2.02	±.31	
Neutral	2.04	±.26	
Difference between means of critical and neutral words	.30	±.20	Critical Ratio..1.53

Intercorrelations

	r	PE_r
Critical vs. Post-critical	+.86	±.03
Critical vs. Neutral	+.79	±.05
Post-critical vs. Neutral	+.77	±.05

The correlations reported with each of the measures taken were all positive, and those for the Luria measures were quite high, ranging from $+ .77 \pm .05$ to $+ .94 \pm .01$. This simply means that the performance of each subject tended to be characterized by a certain level of stability which revealed itself throughout the test, on all word groups; so that the subject who was higher than average in the amplitude of disturbances on the critical words also tended to score higher than the average on the neutral and post-critical word groups. These high coefficients of correlation, together with the high

critical ratios on two of the measures may be taken as evidence of the reliability of the test. Especially in this connection it should be noted that the neutral and post-critical words scored practically the same on all measures, so that presumably the post-critical words might all be considered neutral. Hence the correlations between post-critical and neutral groups might be considered purely as reliability coefficients, and as such indicate fairly high reliability.

Discussion

As was stated at the beginning of this report, the object of this study was to determine the extent to which the phenomena observed by Luria were susceptible to objective, quantitative treatment. We find that the type of disturbance upon which Luria has laid the most stress—impulsive pressures during the latent period—proved itself just as impressive under statistical treatment as it was under the inspectional treatment of Luria; and this in spite of a modified apparatus, a different type of critical word, and subjects much less promising from the standpoint of emotional lability than the criminals, neurotics and overwrought students of Luria's experiments. We are of the opinion that the technique deserves far more attention than it has received thus far as an instrument of research. Whether or not the theory which Luria develops to explain the phenomena he observed is correct, the present study has at least demonstrated that the phenomena themselves are not only observable but amenable to statistical treatment.

Summary

A list of fifty words, fifteen of which were 'critical,' was presented in a free-association situation to thirty-three college students selected at random. The 'critical' words were for the most part words having some sexual connotation. The non-critical words were taken from the Kent-Rosanoff list, and from the non-critical words on Crosland's list. Simultaneously with the usual verbal response, the subjects were required to exert pressure with the preferred hand upon

a recording lever. They were asked to maintain an even pressure upon a second lever with the other hand throughout the experiment. Critical words were differentiated from non-critical words (1) by longer reaction times ($D/\sigma_D = 4.68$), (2) by more intense deflections in the record of the preferred hand during the latent period ($D/\sigma_D = 4.16$), (3) by more intense deflections in the record of the preferred hand *following* the required pressure ($D/\sigma_D = 2.71$) and (4) by more intense deflections in the record of the non-preferred hand ($D/\sigma_D = 1.53$). Post-critical words were accompanied by none of the above-mentioned symptoms, appearing in all respects to be neutral stimuli.

(Manuscript received September 26, 1936)

References

1. Barnacle, C. H., Ebaugh, F. G., and Lemere, F., Association-motor investigation of the psychoneuroses, *Amer. J. Psychiat.*, 1935, **91**, 925–937.
2. Burtt, H. E., Motor concomitants of the association reaction, J. Exper. Psychol., 1936, **19**, 51–64.
3. Crosland, H. R., The psychological methods of word-association and reaction-time as tests of deception, *Univ. Oregon Publ. Psychol. Series*, 1929, **1**, No. 1, pp. 104.
4. Crosland, H. R., and Beck, L. F., Objective measurements of emotion: a symposium of three papers, *Univ. Oregon Publ., Psychol. Series*, 1931, **1**, No. 3, 133–202.
5. Crosland, H. R., and Swartz, B. K., The effects on reaction time of various orders and distributions of relevant stimuli in the word-association experiment, *J. Gen. Psychol.*, 1933, **8**, 217–271.
6. Gardner, J. W., *An experimental study of the Luria technique.* M.A. Thesis, Stanford University, 1936.
7. Hubbard, L. M., Complex-signs in diagnostic free association, J. Exper. Psychol., 1924, **7**, 342–357.
8. Hull, C. L., and Lugoff, L. S., Complex-signs in diagnostic free-association, J. Exper. Psychol., 1921, **4**, 111–136.
9. Huston, P. E., Shakow, D., and Erickson, M. H., A study of hypnotically induced complexes by means of the Luria technique, *J. Gen. Psychol.*, 1934, **11**, 65–97.
10. Jung, C. G. *Studies in word-association* (trans. by M. D. Eder). N. Y.: Dodd, Mead & Co., 1918, pp. 575.
11. Kent, G. H., and Rosanoff, A. J. A study of association in insanity, *Amer. J. Insan.*, 1910, **67**, 37–96, 317–390.
12. Lebedinski, M. S., and Luria, A. R., Die Methode der abbildenden Motorik in der Untersuchung der Nervenkranken, *Arch. f. Psychiat.*, 1929, **87**, 471–497.

13. Luria, A. R., Die Methode der abbildenden Motorik bei Kommunikation des Systeme und ihre Anwendung auf die Affektspsychologie, *Psychol. Forsch.*, 1929, **12**, 127–179.
14. Luria, A. R., Die Methode der abbildenden Motorik in der Tatbestands-diagnostik, *Zsch. f. angew. Psychol.*, 1930, **35**, 139–183.
15. Luria, A. R. *The nature of human conflict* (trans. by W. H. Gantt). N. Y.: Liveright. 1932, pp. xvii, + 431.
16. Olson, D. M., and Jones, V., An objective measure of emotionally-toned attitudes, *J. Genet. Psychol.*, 1931, **39**, 174–196.
17. Rosanoff, A. J. *Manual of psychiatry.* New York: Wiley, 1927, pp. v + 698.
18. Wells, F. L. *Mental tests in clinical practice.* Yonkers, N. Y.: World Book Co., 1927, pp. x + 315.